

# The added worker effect: Evidence from a disability insurance reform\*

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## Abstract

The Netherlands reformed its disability insurance (DI) scheme in 2006. Reintegration incentives for employers became stronger, access to DI benefits became more difficult, or benefits became less generous. Using administrative data on all individuals who fell sick shortly before and after the reform, we study the impact of the reform on labor participation of individuals who fell sick and their spouses. Difference-in-differences estimates show, among other things, that the reform led to an increase of labor participation of the individuals who fell sick only if they had a permanent job, whereas spouses respond to the DI reform in other cases, where the individuals reporting sick had a temporary job or were unemployed. More generally, the spouses respond when the sick individual's labour market position is weak and the individual him- or herself has trouble finding or retaining employment. The effects are persistent during the ten years after the reform. The effect on the spouse can be seen as an "added worker effect," where additional earnings of the spouse compensate for the sick individual's income loss so that both partners share the burden of a more stringent DI scheme. Comparing individuals reporting sick with and without partner provides further support for the notion that the responses of couples to the reform are joint decisions of the two partners.

## 1 Introduction

A large and growing strand of the literature analyzes income complementarities in the household as an insurance mechanism. The "added worker effect" hypothesis suggests that married women

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respond to a negative shock on their husbands' earnings due to unemployment by increasing their hours of paid work (Lundberg, 1985). Most studies find no or a small added worker effect (Maloney, 1987, 1991; Spletzer, 1997; García-Gómez et al., 2012; Bredtmann et al., 2018; Halla et al., 2020; Cammeraat et al., 2023; Jolly and Theodoropoulos, 2023). One explanation is that the affected partner is insured through social insurance so that the spouse does not need to respond (Cullen and Gruber, 2000; Bentolila and Ichino, 2008). Couples may also self-insure through savings and run down their wealth in response to a negative income shock (Blundell et al., 2016). Similarly, the wife's response may be small if the husband's unemployment only leads to a transitory reduction in earnings (Cullen and Gruber; Bredtmann et al.) or if the husband's unemployment is anticipated by the household and the expected income loss already led to adjustments in household consumption and labor supply. In addition, the wife's response will depend on the magnitude of the expected loss in lifetime income (Cullen and Gruber; Stephens, 2002; Bredtmann et al.). An alternative explanation is that the wife's employment prospects may be affected by the factors causing the husband's unemployment (Cullen and Gruber).

Some recent studies, however, do find a notable added worker effect. Ayhan (2018) finds that the probability of a woman participating in the labor force increases by up to 28% in response to her husband's unemployment, although only for two quarters. Schøne and Strøm (2021) find that the rise in wives' labor supply annihilates around one third of the loss in husbands' earnings. Moreover, Blundell et al. show that of the total amount of consumption insured against permanent shocks to the husband's wage, about 63% comes from family labor supply.

In this paper we investigate the existence of an added worker effect in the context of a DI reform that limited DI eligibility. The DI context is interesting for several reasons. First, the number of DI recipients is large and growing in many countries, creating an important challenge for social security funding (OECD, 2018). Moreover, workers who lose income due to the reform have health problems limiting their possibilities to work and recover the income loss themselves – and the income loss is more likely to be permanent than in case of unemployment, which is often temporary. Finally, the reform weakens protection from social insurance, raising the need for self-insurance of the household, for example through a spousal response. Indeed, Bredtmann et al. (2018) show that added worker effects are larger in countries with less protection from social insurance schemes.

In the Netherlands, the share of people receiving DI benefits in the insured population reached about 11%, with almost one million DI recipients in 2002 (Koning and Lindeboom, 2015). To reduce this number and promote work resumption, successive governments implemented several DI reforms. In 2006, the Work and Income According to Labor Capacity Act (WIA) came into effect as the final element of these reforms. WIA introduced major changes in both the DI scheme and the sickness insurance (SI) scheme preceding it, making it much more difficult to become eligible for and to stay on DI benefits. WIA introduced stricter entry criteria for DI and stronger incentives for work resumption, both for employees and employers.

Kantarci et al. (2023) analyzed the effects of the WIA reform on labor participation and benefit receipt among long-term sick individuals (with and without partner) who are unable to perform their work because of occupational or nonoccupational illness or injury. They found that the reform from transitional WAO to WIA substantially reduced the probability of DI receipt during the first ten years after the reform. They also found a rise in labor participation and in unemployment benefits receipt that adds up to almost half of the fall in DI receipt. The labor participation response was particularly strong for those who had a permanent contract when they fell sick and had more possibilities to go back to work than those who had a temporary contract or were unemployed. Since couples can pool income risk, spousal labor supply can be an insurance mechanism to compensate for the loss of DI benefits, particularly if the individual

who fell sick does not manage to go back to work. If the spouse needs to provide care, however, his or her labor supply response to the income shock might be dampened.

The current paper therefore focuses on whether spouses also responded to the reform – and how such a response varied depending on the labor market position of the individual who fell sick. Taking a difference-in-differences approach, we analyze the reform effects on not only the individuals who fell sick but also on their spouses, focusing on heterogeneity of the effects: For individuals with a weaker initial labor market position, i.e., fewer opportunities to go back to work after recovery, there is a larger need for the spouse to compensate for the more stringent rules of the new DI system. Sick individuals who had a permanent work contract at the time of reporting sick increased labor participation, and indeed we find that their spouses did not respond. On the other hand, the fact that sick individuals who had a temporary work contract did not manage to increase labor participation, induced a substantial rise in their spouses labor participation. Similarly, if sick individuals had a weaker labor market position in the sense that they worked in a sector with a low vacancy rate or earned a low wage, the sick individuals themselves hardly responded but their spouses' labor participation rose substantially. These effects are persistent over a period of ten years after reporting sick. Findings for other outcomes (earnings, UI benefits) confirm that the spouse's response is larger in case of a weaker labor market position of the sick individual.

Finally, we compare with the reform effects of sick individuals who have a spouse with the effects on those who do not have a spouse. If there is no spouse who could compensate the loss of household income, the reform raises labor participation of sick individuals with a weak labor market position much more than if there is a spouse. This is in line with our main finding that in couples, the negative income effect of the DI reform is shared by the two partners – single sick individuals cannot rely on their partner to and make a greater effort themselves to resume work.

Our findings add to the limited evidence for the added worker, but also contribute to the literature on the impact of DI reforms. This literature analyses the effects of screening process and eligibility criteria (Karlström et al., 2008; De Jong et al., 2011; Staubli, 2011; Moore, 2015; Autor et al., 2016; Hullegie and Koning, 2018; Godard et al., 2022), benefit generosity (Gruber, 2000; Campolieti, 2004; Marie and Vall Castello, 2012; Mullen and Staubli, 2016; Deuchert and Eugster, 2019), and return-to-work incentives (Kostøl and Mogstad, 2014; Koning and van Sonsbeek, 2017; Ruh and Staubli, 2019; Zaresani, 2018, 2020). It also studies welfare effects (Low and Pistaferri, 2015; Deshpande, 2016; Fevang et al., 2017). None of these studies consider spillover effects on the spouse. Our findings suggest that for a complete evaluation of the DI reform, it is important to consider such spill-over effects on both labor participation and the adequacy of household income.

At the intersection of the literature on the added worker effect and the impact of DI reforms are a few studies that analyze spousal labor supply responses when sick individuals receive DI benefits or eligibility rules for DI benefits change. Results of Duggan et al. (2010) suggest that reform in the US disability program for veterans increasing enrollment, somewhat reduced their wives' labor supply. Borghans et al. (2014) studied the impact of reassessing existing Dutch DI recipients and new applicants younger than 45 years based on new DI eligibility criteria. They found that affected individuals increased their earnings and social support income, but they found no significant effect on spousal earnings. Autor et al. (2019) analyzed the consequences of DI receipt for labor supply and consumption decisions in Norway. They showed that DI denial has little impact on income and consumption of married couples since spousal earnings and benefit substitution counteract the effect of denial of DI benefits. García-Mandicó et al. (2021) analyzed the impact of reassessment of earnings capacity under more stringent rules introduced in 2004. They found that earnings responses of the DI recipient and the spouse

together almost fully compensate for the cut in DI benefits.

Our study differs from these studies in several respects. First, the DI reform in 2006 differs from the reforms studied earlier, restricting access to DI for a large group of workers, and therefore possibly leading to a stronger need for a spousal response. Moreover, due to the administrative nature of our data, we have enough statistical power to analyze heterogeneity in the response. This allows us to show that job security, employment opportunities, and earnings level of the sick spouse are important for the reform effects on both spouses. We also show that the added worker effect is evident for both wives and husbands whereas earlier studies focus on wives' responses to husbands' income shock. Moreover, unlike earlier studies, we also compare with singles, which helps to validate our finding that partners share the burden of the more stringent disability scheme after the reform.

This paper proceeds as follows. Section 2 explains the 2006 reform. Section 3 describes the data and the study sample. Section 4 gives descriptive evidence on the impact of the reform on spousal labor supply. Section 5 presents the empirical approach used to identify the effect of the reform. Section 6 discusses the results for couples and Section 7 compares with the reform effects on singles. Section 8 conducts checks on the identifying assumptions. Section 9 concludes. Appendices include extended analyses.

## 2 Disability insurance in the Netherlands and the 2006 reform

The Dutch system of sickness and disability insurance protects against earnings loss due to incapacity for work and consists of a sickness scheme for the short term and a succeeding disability scheme for the long term. During SI, among others, the development of the health condition and whether reintegration obligations are met are monitored by a certified company doctor from a private occupational health and safety firm. Only after 2 years can a sick worker apply for the public DI benefit. During the assessment for DI, a formal diagnosis is made and work limitations are determined. The loss of earnings of the sick worker is determined by comparing the pre-sickness wage to the potential wage of the sick worker. Potential wage is the median of the highest wages the sick worker could still earn in three jobs. The jobs are selected from a representative sample of jobs in the Dutch labor market, matching the job capabilities and limitations of the sick worker. The loss of earnings as a percentage of the pre-sickness wage determines the disability grade. A minimum disability grade of 35% is required to qualify for the DI benefit, and a disability grade of 80% or more defines full disability and qualifies for a full benefit. The public disability scheme is funded by employer's contributions, mostly flat rate, but partially also differentiated by DI risk.

Uniquely in the world, the Dutch DI scheme covers all causes of sickness, both occupational and social risks, uniformly. However, a fundamental difference exists in provision of SI to workers with permanent and temporary contracts. While the employer's responsibility lasts for the entire two-years period of sickness in case of a permanent contract, it lasts only until the contract ends in case of a temporary contract. Moreover, for workers employed through temporary work agencies, the contract ends as soon as the worker reports being sick. For workers with a permanent work contract, when sickness is reported, the employer is obliged to pay at least 70% of the pre-sickness wage for a period of at most 2 years.<sup>1</sup> Workers with a temporary contract, those employed through a temporary work agency, and those who are entitled to UI have no employer to continue the wage payment and they are eligible for a sickness benefit of 70% of the pre-sickness wage from the Employment Insurance Agency. In fact, the Employment Insurance Agency takes over the role of the employer, both in paying benefits and facilitating

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<sup>1</sup>Most employers pay the full amount during the first year of sickness. There is no formal benefit provision.

reintegration.

The Disability Insurance Act (WAO) is the DI scheme preceding the current DI scheme and was introduced in 1967 to provide compulsory public insurance against loss of earnings due to long-term work incapacity, independent of the cause of the disability. During the late 1970s and 1980s the number of DI beneficiaries rose rapidly to levels far beyond earlier expectations. Entry to the scheme was relatively easy because few reintegration incentives existed during sickness and DI applications were often accepted in case of doubt. Moreover, exit from the scheme was neither incentivized nor closely monitored. As such, there was a substantial share of hidden unemployment in the DI scheme ([Koning and van Vuuren, 2007](#)).

Although major amendments were implemented in 1993, the WAO preserved its main features until 2006. The annual inflow rate into WAO rose to 1.5% of the insured working population in 2001, leading to further reforms. In April 2002 the “Gatekeeper Protocol” was introduced, in which clear and concrete mutual obligations of employers and sick employees for reintegration during the sickness period were specified. A transitional WAO scheme was introduced on 1 October 2004 for people who reported sick between 1 October 2003 and 1 January 2004, making entry criteria stricter. In particular, it adapted a broader definition of the work that the applicant could still do. For example, a sick part-time worker was now supposed to be able to accept a full-time job given the limitations of sickness, and a sick worker who did not speak Dutch was supposed to learn the language to qualify for jobs requiring understanding the Dutch language. As a result, the estimated wage loss due to disability was reduced, making it harder to reach the minimum disability grade to qualify for DI or to reach a higher disability grade (with a higher benefit).

The current Work and Income According to Labour Capacity Act (WIA) was introduced in 2006 for people who reported sick from 1 January 2004 onwards. It introduced major changes in both sickness and disability schemes, stimulating work resumption. It reduced the annual inflow rate into DI to 0.5% of the insured working population during the first six years after its introduction ([Koning and Lindeboom, 2015](#)).

WIA extended the duration of the sickness scheme from one to two years, implying an extension of two main incentives: First, the employer is obliged to compensate the employee for 70% of the wage loss during the additional year in the sickness scheme, creating a strong incentive for the employer to facilitate work resumption. Second, the Gatekeeper protocol was extended to a second year of sickness ([Hullegie and Koning, 2018](#)).

For the disability scheme, WIA kept the stricter DI eligibility criteria of the transitional WAO scheme with the broader definition of what work can still be done. It introduced three other changes. First, the minimum disability grade for entering the scheme rose from 15 to 35 percent – workers with limited disability are expected to resume working (with adaptations of their work if necessary) or apply for UI. Second, the scheme introduced a work resumption program providing strong financial incentives for partially disabled people to utilize remaining work capacity. Third, experience rating for employers was extended from 5 to 10 years, implying that employers are penalized with higher DI premiums if they incurred disability costs for up to five additional years. At the same time experience rating was restricted to partially disabled workers and abolished for permanently and fully disabled workers. Targeting the former group made experience rating more effective since the partially or temporary disabled have better prospects of reintegration. Experience rating was limited to permanent work contracts until 2013 and extended to temporary contracts afterwards. Figure 4 in Appendix A presents a timeline of changes in the DI scheme starting from the introduction of the WAO in 1967 until the extension of experience rating to temporary contracts in 2013.

For the income of the sick individuals during sickness and disability periods, potential implications of the WIA reform are as follows. In the first year of sickness, wages are not affected

by the reform. However, employers may already do more for reintegration in the first year of sickness if they anticipate the cost of the additional year of wage payments. These stronger employer incentives may induce sick individuals to return to work, especially in combination with the requirements of the Gatekeeper protocol. On the other hand, reintegration incentives for employees might have become weaker in the first year since employees are no longer subject to a DI assessment after one year of sickness.

In the second year of sickness, WIA requires that the employer replaces (at least) 70% of the former wage. In WAO, DI and UI benefits together replaced 70% of the former wage. From the third year onwards, a potential fall in income is due to lower or a complete loss of DI benefits. As described above, this owes to the stricter eligibility criteria for DI, financial incentives for work resumption, and extended and more targeted reintegration incentives of experience rating. Note that these implications of the reform assume that the sick individual has a stable work contract with an employer. Employees with weak employer relationships or those who are unemployed will lack the reform incentives and may struggle to resume work and cope with the negative income shock of the reform. They may seek alternative welfare benefits, or rely on the income of their spouse.

### 3 Data

We use unique administrative data from the Employee Insurance Agency on all individuals who fell sick in the fourth quarter of 2003 or the first quarter of 2004, and therefore could become eligible to either the transitional WAO or the WIA scheme. We observe the beginning and ending dates of their sickness, their gender, date of birth, and sector of economic activity. They either earned a wage or receive UI at the time they report sick – other groups cannot enter the sickness scheme. For wage earners, we observe whether they had a permanent contract, a temporary contract, or a contract through a temporary work agency at the time they reported sick. We link these individuals to administrative data on themselves and their partners (married or cohabiting) from Statistics Netherlands (CBS), with monthly information on wages and benefits (including DI and UI) from January 1999 to February 2014.

The initial data set has 171,281 individuals reporting sick. To select the estimation sample, we drop individuals who participate in the special disability schemes for the self-employed or for young people, since the rules and incentives for them are quite different. We also drop individuals who already received DI when they reported sick. We drop individuals in same-sex partnerships and only keep couples if their cohabitation started before reporting sick. We drop individuals whose spouse also reported sick between October 2003 and March 2004. Finally, we only keep those who spent more than 90 days in sickness leave, since employers only have to report sickness cases if they last longer than 90 days.<sup>2</sup> We divide the sample into a “control group” of individuals (and their spouses) who fell sick in the fourth quarter of 2003 and were insured under the transitional WAO scheme and a “treatment group” of individuals (and their spouses) who reported sick in the first quarter of 2004 and were insured under the WIA scheme. We will not consider individuals who reported sick before October 1 2003 and fall under the old WAO scheme and refer to the transitional WAO group as WAO group from now on.

Based on the available data on wages and social security benefits, we define the following outcome variables: dummies that indicate labor participation and UI receipt, and the monthly amounts of wages and UI benefits. We transform earnings and benefit amounts as the natural logarithm of the amount plus 1, accounting for the skewed distribution and the zero values. During participation in the sickness scheme, the observed wage combines two types of payments:

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<sup>2</sup>temporary work agencies have to report all sickness cases.

earnings (for the part of work capacity that is still used) and compensation for lost earnings due to sickness benefits paid by the employer. We do not observe the separate amounts. Since we measure labor participation as positive earnings, this implies that we cannot determine whether or not sick people are working when in the sickness scheme. We therefore discard the first two years after individuals reported sick in most of our analysis. After the first two years, SI expires for everyone and measuring labor force participation is no longer problematic.

## 4 Time trends and other descriptive statistics

Figure 1 shows the labor participation rates and fractions of DI and UI recipients in control and treatment groups over the observation period.<sup>3</sup> For the individuals in our data who all reported sick, DI benefit receipt increases sharply when they become eligible for DI benefits and continues to increase during the remaining years of the observation period. The WIA group is 3 pp less likely to receive DI benefits than the control group (13.5% versus 10.5%) and the difference between the two groups remains stable till the end of the observation period. This shows that the reform effectively limited access to DI benefits. For the spouses of sick people, DI receipt is stable and not affected by the reform (as expected).

For individuals who reported sick, the probability of working shows a strong time trend that is common to both groups. It increases until the date individuals report sick, reflecting that individuals can enter the sickness scheme only if they are working or receive UI. Before this, they can have another labor force status. The probability of working falls sharply during the first few years of sickness and continues to fall throughout the remaining years. The difference between WAO and WIA groups is small and insignificant before individuals fall sick, but notable and significant after that, suggesting that the reform increased labor participation of those who fell sick. For spouses, the probability of working shows a less pronounced decreasing pattern. The difference between groups is insignificant before and after treatment, but it is larger post-than pre-treatment, suggesting that there might be a positive spill-over effect.

For sick individuals in both groups, the use of UI falls sharply right after reporting sick, since those who are unemployed replace UI with sickness benefits. UI use rebounds and increases during the remaining months of the sickness scheme, since many individuals recover and replace their sickness benefit with UI. UI use peaks when individuals can apply for DI, since rejected DI applicants turn to UI when the sickness period ends. UI use falls during the disability period because UI is temporary with a maximum of 38 months. The difference between the control and treatment groups is sizable and statistically significant during the disability period, suggesting that the DI reform increased UI use among those who reported sick. UI use among the spouses is fairly constant over time. The difference between control and treatment groups is insignificant, both pre- and post-treatment.

Table 1a presents sample means of some background characteristics when reporting sick for both groups, as well as outcomes before and after reporting sick. It also presents tests for equality of the means in control and treatment groups (“balancing tests”). Panel A shows that, in both groups, the average age is about 43 and there are more men than women. The majority held a permanent work contract when they fell sick; the others had a temporary contract or a contract through a temporary work agency, or were unemployed. Column 3 shows that there are small but significant differences between the treatment and control group. These possibly reflect labour market trends. Our identification strategy (difference-in-differences) accounts for such differences.

Columns 3 and 6 in panel B present mean differences in outcomes during the pre- and post-

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<sup>3</sup>Similar figures for wages and benefit amounts (not shown) reveal very similar patterns.

treatment periods for treatment and control group. The fraction of sick individuals receiving disability benefits falls due to the reform, as expected. In line with Figure 1, the difference is larger post- than pre-treatment for all outcomes, again suggesting that the reform has increased labor participation, average earnings, UI receipt, and the average amount of UI benefits.

Table 1b reproduces Table 1a for the spouses. Spouses in the treatment group are slightly older than the control group. Couples in the treatment group have cohabited somewhat longer pre-treatment but not post-treatment. Columns 3 and 6 in panel B show that the difference in labor participation between groups is larger post-treatment than pre-treatment, which, again, might suggest that the reform increased labor participation for the spouses. The mean differences in other outcomes are small and insignificant, both pre- and post-treatment.

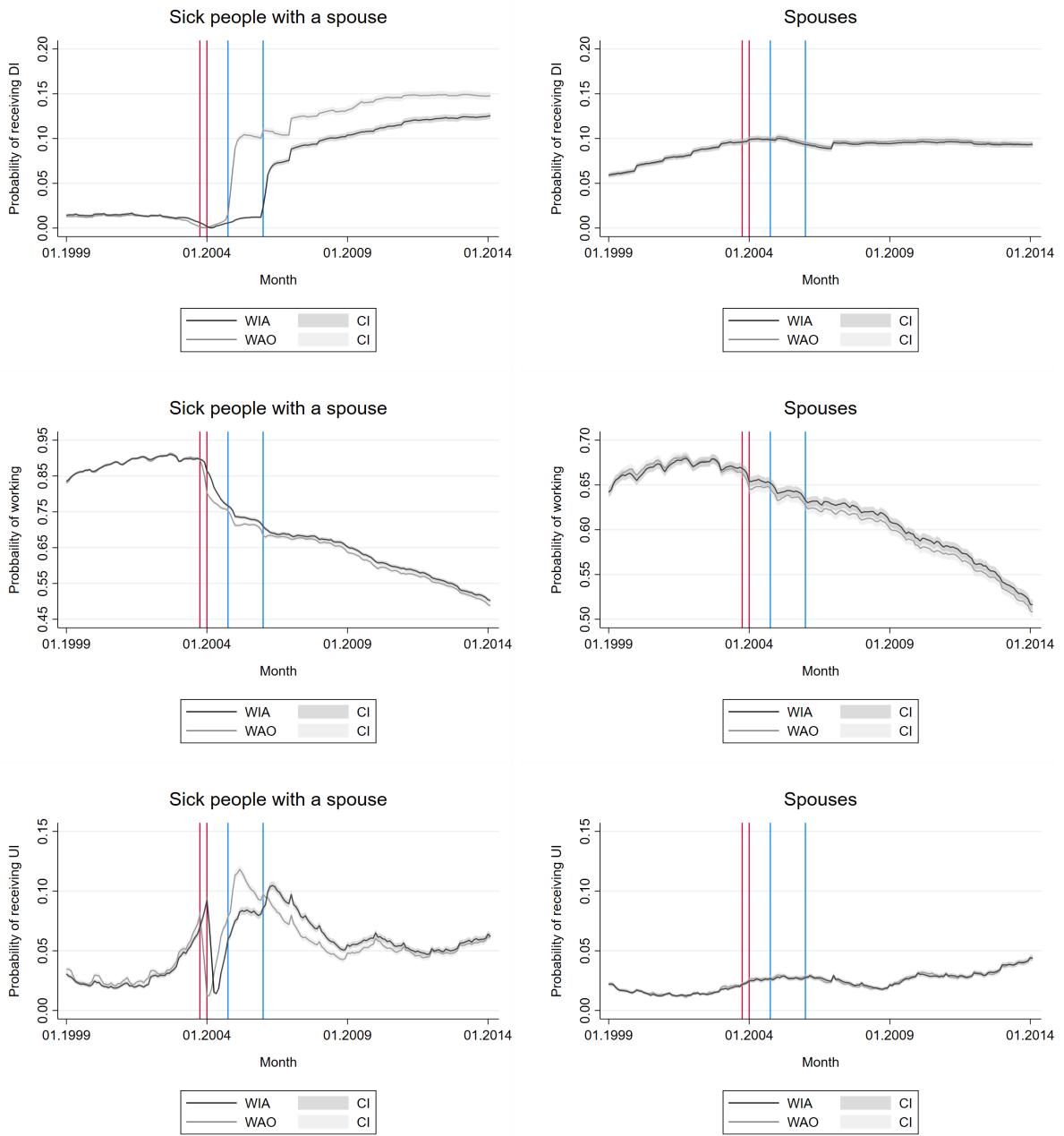


Figure 1: Probability of DI receipt, working, and UI receipt for control and treatment groups by calendar month; sick individuals (left) and their spouses (right). Vertical lines mark the first instance sick individuals can be entitled to sickness (red) and disability (blue) benefits.

Table 1a: Sample means and balancing tests of background characteristics and outcome in control and treatment groups before and after sickness for sick individuals with a partner

	Before		After		
	WAO group	WIA group	WIA and WAO	WAO group	WIA group
	(1)	(2)	(3)	(4)	(5)
<b>A. Background characteristics</b>					
Age	42.946	43.108		0.162*	
Female	0.379	0.384		0.005	
Permanent contract	0.705	0.717		0.012***	
Temporary contract	0.123	0.106		-0.017***	
Unemployed	0.171	0.176		0.005*	
<b>B. Labor market outcomes</b>					
DI (possibly UI) receipt				0.135	0.106
Labor participation	0.887	0.889	0.002	0.611	0.615
UI (no DI) receipt	0.033	0.033	0.000	0.057	0.063
DI (and possibly UI) per month				187.298	162.690
Wage per month	1,999.228	2,012.334	13.106	1,713.940	1,738.323
UI (excl. DI) per month	39.938	40.128	0.190	89.747	92.873
Observations	1,589,880	1,716,480		2,543,808	2,746,368
Individuals	26,498	28,608		26,498	28,608

Notes: 1. “Before”: period before individuals fall sick (January 1999 - October 2003 for individuals who fell sick in November 2003; January 1999 - January 2004 for individuals who fell sick in February 2004). “After”: period after individuals fell sick excluding the first two years (November 2005 - January 2014 for individuals who fell sick in November 2003; February 2006 - January 2014 for individuals who fell sick in February 2004). 2. Age is at the time individuals fall sick. “Permanent contract”, “temporary contract”, and “unemployed” refer to labor market status of individuals when they fell sick. 3. Columns 1, 2, 4 and 5 present means in control (WAO) and treatment (WIA) group before and after start of sickness. Columns 3 and 6 present differences between individuals insured under WIA and WAO – the estimated coefficient from the regression of the characteristic or outcome as the dependent variable, and an indicator of participation in WIA as the explanatory variable. Standard errors clustered at the individual level.

Table 1b: Sample means and balancing tests of background characteristics and the outcome in control and treatment before and after sickness for spouses

	Before		After		Diff. WIA and WAO (6)
	WAO	WIA group	Dif. WIA and WAO (3)	WAO group (4)	
	(1)	(2)	(5)	(6)	
<b>A. Background characteristics</b>					
Age	42.333	42.535	0.205**		
Years of cohabitation	7.031	7.173	0.142***	8.269	8.224
<b>B. Labor market outcomes</b>					
DI (possibly UI) receipt	0.670	0.669	-0.001	0.096	0.094
Labor participation	0.016	0.016	0.000	0.586	0.590
UI (no DI) receipt				0.027	0.028
DI (and possibly UI) received per month				115.472	113.790
Wage per month	1,299.714	1,312.522	12.808	1,552.520	1,561.632
UI (excl. DI) per month	18.356	19.220	0.864	38.291	38.761
Observations	1,589,880	1,716,480		2,543,808	2,746,368
Individuals	26,498	28,608		26,498	28,608

Notes: 1. “Before”: period before individuals fall sick (January 1999 - October 2003 for individuals who fell sick in November 2003; January 1999 - January 2004 for individuals who fell sick in February 2004). “After”: period after individuals fell sick excluding the first two years (November 2005 - January 2014 for individuals who fell sick in November 2003; February 2006 - January 2014 for individuals who fell sick in February 2004). 2. Age is at the time individuals fall sick. Years of cohabitation for the “Before” period indicates mean years of cohabitation by the time individuals fall sick. That for the “After” period indicates mean years of cohabitation during the period after individuals fell sick including the first two years. 3. Columns 1, 2, 4 and 5 present means in control and treatment before and after start of sickness. Columns 3 and 6 present differences between individuals insured under the WIA and WAO – the estimated coefficient from the regression of the characteristic or outcome as the dependent variable, and an indicator of participation in the WIA as the explanatory variable. Standard errors clustered at the individual level.

## 5 Identification strategy

We use a difference-in-differences approach to identify the causal effect of the WIA reform on each outcome variable  $y_{it}$ , either concerning the sick individual or the spouse. The first difference is across groups. Those who reported sick in the first quarter of 2004 (treatment or WIA group) face different eligibility criteria and incentives to work or claim benefits than individuals who reported sick in the fourth quarter of 2003 (control or WAO group). The second difference refers to event time: before and after reporting sick.

We start the DiD comparison using the following baseline regression model:

$$y_{it} = \alpha_i + \gamma (Treated_i \times Post_t) + \delta Post_t + \lambda_{s(i,t)} + \varepsilon_{it}. \quad (1)$$

Here  $i$  indexes the sick individual or their spouse.  $t$  indexes the month of event time: Values  $-57$  to  $-1$  indicate the months before reporting sick,  $0$  is the month when first reporting sick, and  $1$  to  $119$  are the months after reporting sick. (For some outcomes  $y_{it}$ , we do not use observations during the sickness period due to measurement issues; see Section 3).  $\lambda_{s(i,t)}$  is a monthly calendar time effect –  $s(i,t)$  indexes the calendar month (from January 1999 until February 2014; January 1999 is chosen as the base month) for individual  $i$  at a given month of event time  $t$ .  $\alpha_i$  is an individual-specific, time-invariant fixed effect that is potentially correlated with the control variables.  $\varepsilon_{it}$  represents an idiosyncratic (unobserved) shock, assumed to be uncorrelated with all the explanatory variables.

$Treated_i$  is a dummy variable for the treatment (WIA) group.<sup>4</sup>  $Post_t$  is an event time dummy with value  $1$  from the start of the sickness period. The individual effects capture differences between the two groups other than the reform effect. Under the identifying assumption that treatment and control group would have followed the same trend if there would not have been a reform, the coefficient  $\gamma$  on the interaction term  $Treated_i \times Post_t$  captures the effect of the reform, the parameter of interest.<sup>5</sup>

To disentangle the effect of the WIA reform in the short and long run, and test for the common trend assumption, we consider the following extended model:

$$y_{it} = \alpha_i + \sum_{l=-5}^9 \gamma_l (Treated_i \times d_{lt}) + \sum_{l=-5}^9 \delta_l d_{lt} + \lambda_{s(i,t)} + \varepsilon_{it}. \quad (2)$$

Instead of  $Post_t$  which refers to the entire period after falling sick, this model has separate dummies for each year, after and before falling sick.  $d_{lt}$  indicates the  $l$ -th year from the time the individual reports sick. Year  $-1$  is chosen as the base year. The coefficients on the interaction terms of treatment and these year dummies are the estimated treatment effects.<sup>6</sup> For the years before reporting sick, they provide a test of the common trend assumption. For the period after reporting sick they reflect the dynamic effects of the reform. In this setup, treatment and control groups are compared over event time  $t$ , i.e., the months before and after the individual reported sick. The calendar time dummies  $\lambda_{s(i,t)}$  on the other hand capture the (common) calendar time trend.

In Section 6.3, we allow for heterogeneous reform effects depending on the labour market status at the time the individuals reported sick. In particular, we hypothesize that the effects depend on how easy it is for the sick individuals to go back to work (either to their old job or to a new one). In Section 7, we also apply the same model to individuals without a spouse who

<sup>4</sup>Since this is time invariant, it is omitted in the fixed effects regression.

<sup>5</sup>We cannot separately identify the effects of the different components of the reform, i.e. the extension of the sickness period, changes in financial incentives, and stricter eligibility criteria.

<sup>6</sup>Here we also include observations for  $t = 0, \dots, 23$ .

reported sick. This is to investigate whether the sick individuals behave differently if there is a spouse who can potentially respond to the reform by increasing labour supply and household income.

To control for observed differences between treatment and control individuals before reporting sick, we apply entropy balancing following Hainmueller (2012). In particular, individuals are weighted to adjust inequalities in representation with respect to the first moment of the covariate distributions. As covariates, we consider their gender and birth year, as well as all outcomes of the sick individuals and their spouses before the first group reported sick. Regressions of equations (1) and (2) are estimated based on the constructed weights.<sup>7</sup> The weights are regenerated in each subsample when analyzing heterogenous treatment effects. To check whether, after entropy balancing, the common trend assumption is satisfied pre-treatment and to analyze several other threats to our identification strategy, we perform additional analyses and robustness checks in Section 8.

## 6 The effect of the reform on labor participation of sick individuals and their spouses

We first present the effects for the whole post-treatment period (equation (1)), then analyze the short- and long-run effects of the reform (equation (2)), and finally check for heterogeneous effects. We also analyzed the effects of the transitional WAO reform and present the results in Appendix I.

### 6.1 Baseline effects

Table 2 presents the baseline DiD estimates of the reform effects on labor participation and benefit receipt. For the sick individuals, the reform decreased the probability of DI receipt by 3.1 percentage points (pp) on average during the post-treatment period (excluding the first two years). It increased the probability of working by 1 pp and UI receipt by 0.8 pp.<sup>8</sup> The reform induced the spouses of the sick individuals to raise their labor participation by 0.5 pp, one sixth of the drop in DI receipt of the individuals who reported sick, but this effect is significant at the 10% level only.

The center panel of Table 2 shows the DiD estimates of the reform effects on monthly wages and benefits (in log of the amount plus 1). The reform reduced monthly DI benefits of individuals reporting sick by 20.3% and increased monthly earnings by 7.6% and monthly unemployment benefits by 5.6%. Moreover, it increased earnings of spouses by 3.6%, but this increase is not statistically significant.

The lower panel of Table 2 presents the DiD estimates of the reform effects for monthly total income (in log of the amount plus 1), pooling monthly wages and social security benefits (DI, UI and social assistance). It presents the total income of sick individuals and their spouses at the individual level, but also total income of the couple. On average, the reform did not significantly affect total income of sick individuals, their spouses, or their household. The first suggests that in most cases, sick individuals are able to compensate lost disability benefits by increasing earnings and income from UI. This may explain why the spouses' income increase is not that large (or significant) either – there is not much need for a spousal response. In the heterogeneity analysis in Section 6.3, however, we will find that this aggregate result is mainly

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<sup>7</sup>Following Imbens (2004) and using propensity scores to construct weights leads to almost identical estimates.

<sup>8</sup>This is qualitatively in line with what Kantarci et al. (2023) found. The magnitude of the effect on DI receipt is different, mainly because we take everyone who has been sick for at least 90 days whereas Kantarci et al. only consider those who have been sick for 180 days; see the sensitivity analysis in their Appendix B.

due to groups of individuals reporting sick who relatively easily can go back to work and increase their earnings. The aggregate findings fail to show that for individuals reporting sick and who are less likely to compensate lost disability benefits by responding themselves, spousal earnings do respond.

Table 2: Estimated effects of the WIA reform: Sick individuals and their spouses

	Sick individual	Spouse
DI receipt	-0.031*** (0.002)	-0.001 (0.002)
Labor participation	0.010*** (0.003)	0.005* (0.003)
UI receipt	0.008*** (0.001)	0.000 (0.001)
ln DI	-0.203*** (0.018)	-0.011 (0.012)
ln Wage	0.076*** (0.027)	0.036 (0.023)
ln UI	0.056*** (0.009)	0.003 (0.006)
ln Total individual income	-0.010 (0.023)	0.016 (0.022)
ln Total household income	0.009 (0.018)	
Observations	8,431,218	
Individuals	55,106	

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## 6.2 Dynamic effects

Figure 2 presents the estimates of the reform effects separately for ten years of the post-treatment period. For individuals reporting sick, the reform reduced DI receipt by about 3 pp from the third year after reporting sick, when both the treatment and the control group can apply for DI. (Note that the large effect of the reform on labor participation during the first year of the sickness scheme is hard to interpret, due to the measurement issue explained in Section 3.) The effect on labor participation then falls to about 1 pp and remains fairly stable. For UI receipt, the large negative effect in the second year of the sickness scheme is due to the fact that individuals insured under WIA are still entitled to sickness wage payment if there is an employer, or the sickness benefit if there is no employer. From the third post-treatment year onwards, however, the reform has a positive effect on UI receipt. It falls over time and becomes

insignificant from year 8 after reporting sick.<sup>9</sup> In each year after reporting sick, the effect of the reform on spousal labor participation is about 0.5 pp, but this is never significant. Moreover, in line with the exploratory analysis in Section 4, the reform has no significant effect on spouses' UI receipt.

### 6.3 Heterogeneous effects

Analyzing wives' labor supply response to an exogenous shock to husband's job earnings in Austria, Halla et al. (2020) find that the added worker effect by wives is almost negligible in their full sample. To understand the reasons for the limited responses and to identify impediments to the intrahousehold insurance mechanism, they investigate heterogeneity in responses for different types of households with the goal of identifying more and less responsive groups in the overall population. As shown in Table 2, the average added worker effect is also small in our context. This finding, however, may mask interesting heterogeneous effects. Existing studies on the added worker effect discuss a variety of factors that could induce wives to respond to a negative shock on their husbands' earnings. Some consider the nature of the income shock and argue that spouses would respond if the income shock is permanent, unanticipated, or its magnitude is large (Cullen and Gruber, 2000; Stephens, 2002; Blundell et al., 2016; Bredtmann et al., 2018; Fadlon and Nielsen, 2021). Others consider that lack of self-insurance through savings or formal insurance through social support programs, high earnings potential of the wife, and existence of job opportunities for wives may encourage wives to respond (Cullen and Gruber; Bentolila and Ichino, 2008; Blundell et al.; Halla et al., 2020). We hypothesize that when the labour market position of sick workers facing the reform are weak, the sick individual's labour supply response will be weaker and the spousal response will be stronger. We explore three indicators of a weak labour market position: employment status when reporting sick (permanent job, temporary job, or unemployed), earnings level before reporting sick, and the sectoral vacancy rate in the year of reporting sick.<sup>10</sup>

#### Employment status when reporting sick

If sick individuals have temporary work contracts, they may not be able to go back to their job after recovery or find another job. Similarly, workers who reported sick while unemployed may have trouble finding a job when their sickness benefit expires. Furthermore, stimulating employers to increase labor market participation has been a key element of Dutch labor market reforms throughout the years. The WIA reform in particular introduced strong reintegration incentives for employers (see Section 2), but these incentives do not apply uniformly to all workers: Employer incentives for temporary workers only last for the duration of the employment contract. Moreover, temporary work agencies do not face incentives for their sick employees during sickness, since their sickness benefits are paid by the Employee Insurance Agency. Unemployed individuals obviously do not benefit from positive effects of employer incentives either. On the other hand, employers of employees with a permanent contract are fully incentivized due to continued wage payments and experience rating, which applied only to permanent work contracts until 2013. Prinz and Ravesteijn (2020) found that the extension of experience rating to temporary workers reduced DI receipt by 12.7 pp and increased labor participation by 2.5 pp

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<sup>9</sup>Again, these results are qualitatively in line with those of Kantarci et al. (2023), cf. their Figure 4.

<sup>10</sup>The literature on the added worker effect typically focuses on the wife's response to shocks in the husband's income. In contrast to García-Mandicó et al. (2021), who analyzed the impact of the change in reassessment rules in 2004, we find hardly any differences between the spousal effects for men and women (details available upon request).

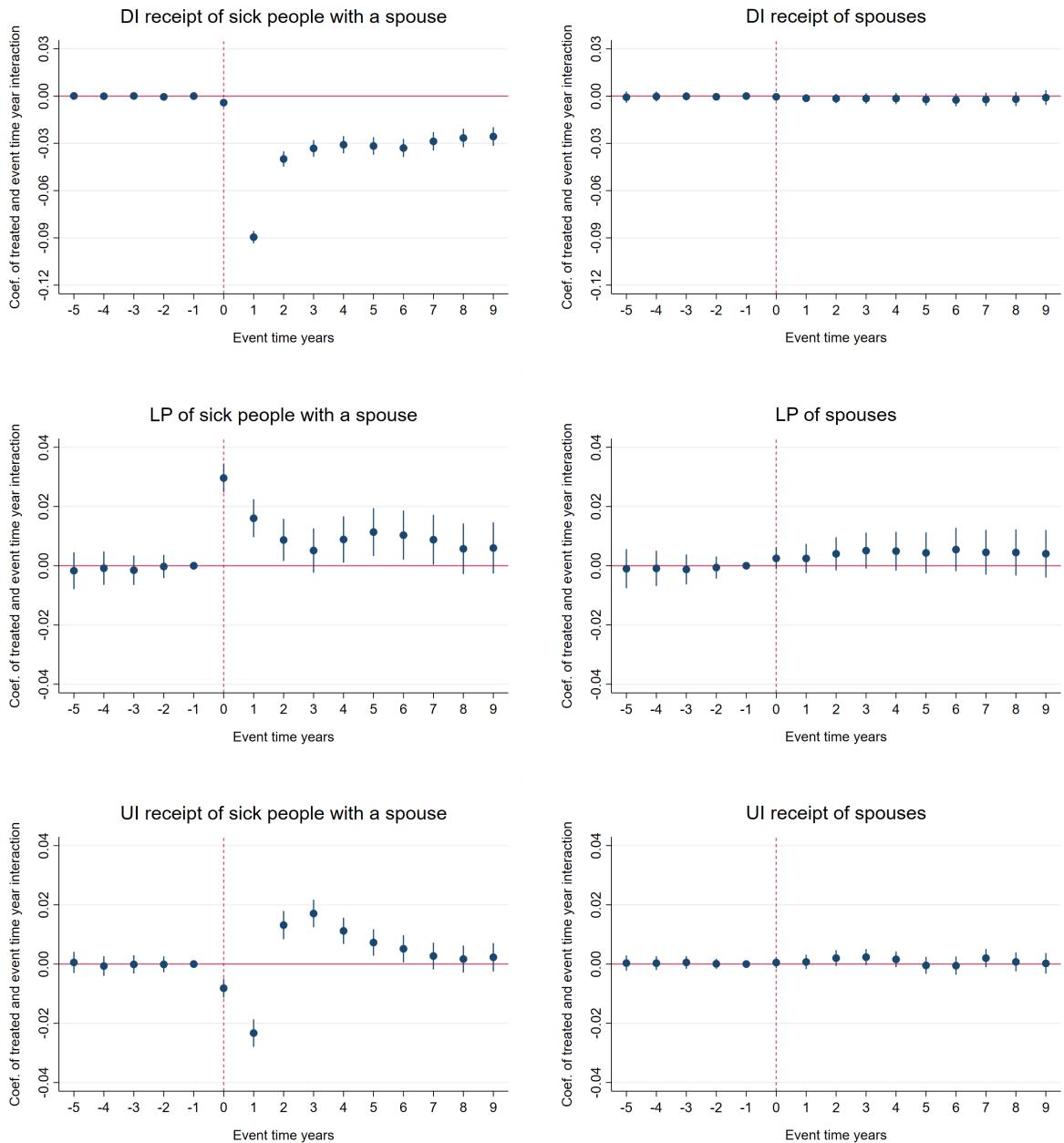


Figure 2: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

among workers with temporary contracts relative to those with permanent contracts. In summary, there are several reasons why those who had a temporary contract or were unemployed when falling sick have more problems to go back to paid work after recovery and more often have to cope with a negative income shock due to the reform. They may then also more often have to rely on their spouse's income, and their spouse may respond more strongly if DI benefits are lost due to the reform.

We separately estimate the reform effects for individuals who were wage earners with a permanent contract, wage earners with a temporary contract, or unemployed at the time they reported sick. The upper panel of Table 3 presents the results, which are largely in line with the hypotheses formulated above. Due to the reform, DI receipt fell substantially for all groups, and the largest fall is for the unemployed, who have no employer that can help them resume work. This increases their chances of remaining in the sickness scheme and facing the stricter requirements of WIA to enter DI.

The reform only increased labor participation among those who reported sick when they had a permanent work contract, even though the fall in DI receipt is larger for the other two groups. It suggests that the reform's work resumption incentives induced employers to reintegrate their permanent employees, but were not effective for temporary contracts or unemployed workers. For the unemployed, the longer sickness period may also lead to more human capital loss or a stronger scarring effect, reducing the prospects of finding a job (Arulampalam, 2001; Arulampalam et al., 2001). Moreover, their incentives to resume work quickly may be reduced by the additional year they can spend in the sickness scheme.

The reform increased UI receipt for all sick individuals, irrespective of their work status. The increase is largest for the unemployed, where UI is usually the primary source of income. The effect for those on a temporary contract is larger than for those on a permanent contract – the former have lower and less stable earnings, and seek additional income from UI if the reform blocks access to DI benefits.

Since sick individuals with a permanent work contract often resume work and increase earnings themselves, their spouses less often need to compensate, and indeed, the labor participation response of the spouses in this group is small and insignificant. On the other hand, the spouses of sick individuals on a temporary contract increased labor participation and earnings significantly and Figure 5 in Appendix B shows that this effect is persistent.<sup>11</sup> Since sick individuals with a temporary contract struggle to resume working, this confirms that their spouses increase labor participation and earnings to compensate for the lost disability benefits and lack of labor income. This added worker effect on labor participation is particularly large – 81% of the drop in DI receipt. The spouses of sick individuals who were unemployed also increase their labor participation by a notable amount of 1 pp, but this estimate is less precise and not significant.

The signs and significance levels of the estimated effects of the reform on earnings and benefit amounts in the upper panel of Table 4 are in line with the estimated effects on labor participation and benefit receipt. The upper right panel of Table 4 presents the effects of the reform on total income (in log of the amount plus 1) of sick individuals and their spouses at the individual and the household level. Individual income of sick individuals who had a temporary contract or were unemployed fell significantly due to the reform. However, due to the positive responses of their spouses, household income does not change significantly, confirming that spouses' responses help to smooth household income. This result is in line with the findings of Blundell et al. (2016) based on a structural family labor supply model where households self-insure through spousal labor supply in case of a negative income shock. For sick individuals with a permanent contract, total income did not change significantly at either the individual or the household level.

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<sup>11</sup>The righthand panel in this figure will be discussed in Section 7.

## Pre-sickness earnings

If sick individuals earn low wages (regardless of their sickness), spouses may respond more strongly for different reasons. Low wage earners tend to have smaller savings or wealth to draw on during sickness to smooth their consumption path. They also tend to work in jobs where prospects of recovery from ill health are limited. If as a result the income shock becomes permanent, spouses may exhibit stronger responses. Furthermore, the Dutch DI scheme is an income insurance scheme. The benefit amount can be different for two people with similar disabilities. The likelihood of receiving DI benefits depends on the wage earned before reporting sick (Section 2). For people with low earnings, the income decline due to disability is often limited. As a result, low-wage earners are much more likely to be denied DI benefits than higher wage earners (OCTAS, 2023). The workforce hit by the DI reform could therefore often include low-wage earners who are likely to be more in need of a spousal response. The pre-sickness earnings measure we use is the average of the individual's earnings during the five years before they reported sick (where data is available). The center panel of Table 3 presents the estimation results by pre-sickness earnings quartile. Sick individuals in lower earnings quartiles increased their UI receipt somewhat more, in line with the argument that they struggle more to find suitable jobs where they can utilize their remaining work capacity and more often have to rely on income from UI. For the lowest two quartiles, we find that spouses notably increase their labor participation, a response which is more than half (52%) of the drop in DI receipt of the sick partner. For the higher quartiles, however, no spousal response is observed. These results are in line with the results based on employment status in the preceding subsection – both suggest that spousal responses are stronger for sick individuals in a weaker labor market position.

## Vacancies in the sector

For sick individuals who have limited employment opportunities and hence a higher risk of unemployment, responding to the work incentives of the DI reform can be more difficult and spousal labor supply responses can be stronger. We consider the sectoral vacancy rate (the number of open vacancies per one thousand jobs) as an indicator of employment opportunities.<sup>12</sup> We distinguish two groups: individuals who at the time of reporting sick worked in sectors with vacancy rates below (e.g., construction, manufacturing, transport, public sector) or above the average vacancy rate (e.g., agriculture, trade, financial services, catering).

The lower panels of Tables 3 and 4 present the results. If sick individuals work in a sector with a vacancy rate below the average, their spouses increase labor participation by 0.9 pp, a sizable extensive margin added worker effect of 23% of the drop in DI receipt by the sick partner who has limited employment opportunities him or herself. In contrast, if sick individuals work in a sector where the vacancy rate is above the average, the reform raises their own labor participation by 1 pp while their spouses do not respond significantly. Again, these results confirm that the added worker effect is a more powerful insurance mechanism when the labor market position of the sick individual affected by the reform is weak.

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<sup>12</sup>The sector where sick individuals are or were employed is available in the sickness data, and we determine the vacancy rate in each sector prior to and in the year of reporting sick using data from Statistics Netherlands.

Table 3: Estimated effects of the WIA reform on DI receipt, labor participation and UI receipt of sick people and their spouses by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	DI receipt		Labor participation		UI receipt	
	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse
On permanent contract	-0.027*** (0.002)	-0.002 (0.002)	0.016*** (0.004)	0.001 (0.004)	0.004*** (0.001)	0.002** (0.001)
On temporary contract	-0.031*** (0.009)	-0.008 (0.006)	-0.015 (0.012)	0.025*** (0.009)	0.013*** (0.004)	-0.002 (0.003)
Unemployed	-0.041*** (0.008)	0.003 (0.005)	-0.014 (0.009)	0.010 (0.008)	0.021*** (0.005)	-0.004 (0.003)
Earnings in 4th quartile	-0.037*** (0.004)	-0.001 (0.003)	0.013* (0.007)	-0.004 (0.006)	0.007*** (0.002)	-0.000 (0.002)
Earnings in 3rd quartile	-0.036*** (0.005)	-0.001 (0.004)	0.004 (0.006)	-0.002 (0.006)	0.007*** (0.002)	0.002 (0.002)
Earnings in 2nd quartile	-0.024*** (0.005)	-0.001 (0.004)	0.010 (0.007)	0.012* (0.006)	0.008*** (0.002)	0.002 (0.002)
Earnings in 1st quartile	-0.025*** (0.006)	-0.000 (0.004)	0.006 (0.007)	0.013** (0.006)	0.011*** (0.003)	-0.001 (0.002)
Vacancy rate above the mean	-0.022*** (0.003)	-0.002 (0.002)	0.010** (0.005)	0.002 (0.004)	0.008*** (0.002)	0.001 (0.001)
Vacancy rate below the mean	-0.039*** (0.004)	-0.000 (0.003)	0.008* (0.005)	0.009** (0.004)	0.008*** (0.002)	0.000 (0.001)

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

Table 4: Estimated effects of the WIA reform on DI benefits, wages, UI benefits and total income of sick people and their spouses by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	In DI		In Wage		In UI		In Total individual income		In Total household income	
	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse	Sick individual	Spouse
On permanent contract	-0.183*** (0.018)	-0.014 (0.014)	0.130*** (0.031)	0.004 (0.027)	0.027*** (0.009)	0.015** (0.007)	0.029 (0.027)	-0.006 (0.026)	0.012 (0.021)	
On temporary contract	-0.196*** (0.067)	-0.057 (0.041)	-0.126 (0.088)	0.182** (0.007)	0.091*** (0.030)	-0.016 (0.019)	-0.180** (0.075)	0.090 (0.071)	-0.017 (0.054)	
Unemployed	-0.265*** (0.059)	0.015 (0.035)	-0.126* (0.070)	0.076 (0.059)	0.148*** (0.033)	-0.030 (0.018)	-0.145** (0.062)	0.070 (0.059)	-0.040 (0.049)	
Earnings in 4th quartile	-0.260*** (0.033)	-0.006 (0.022)	0.121** (0.057)	-0.036 (0.047)	0.051*** (0.016)	-0.000 (0.012)	0.015 (0.050)	-0.029 (0.045)	-0.036 (0.041)	
Earnings in 3rd quartile	-0.232*** (0.035)	-0.004 (0.024)	0.030 (0.052)	-0.008 (0.044)	0.048*** (0.015)	0.014 (0.012)	-0.031 (0.043)	0.003 (0.042)	-0.010 (0.034)	
Earnings in 2nd quartile	-0.152*** (0.036)	-0.013 (0.026)	0.076 (0.052)	0.071 (0.047)	0.057*** (0.017)	0.010 (0.013)	0.017 (0.041)	0.019 (0.046)	0.025 (0.032)	
Earnings in 1st quartile	-0.164*** (0.041)	-0.004 (0.028)	0.039 (0.053)	0.098** (0.049)	0.073*** (0.023)	-0.009 (0.014)	-0.056 (0.051)	0.080* (0.048)	0.032 (0.039)	
Vacancy rate above the mean	-0.145*** (0.025)	-0.018 (0.017)	0.084** (0.038)	0.010 (0.034)	0.058*** (0.013)	0.005 (0.009)	0.037 (0.033)	-0.009 (0.033)	0.025 (0.026)	
Vacancy rate below the mean	-0.258*** (0.026)	-0.002 (0.018)	0.064* (0.039)	0.063* (0.033)	0.060*** (0.013)	0.000 (0.009)	-0.048 (0.034)	0.046 (0.031)	-0.001 (0.027)	

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## 7 Comparing with the reform effects on sick individuals without a spouse

The results in the preceding section suggest that spouses increased their labor participation to compensate for lost disability benefits of their sick partners, particularly if the sick individuals cannot increase their own earnings. Here we compare the reform effects on labor participation of sick individuals with and without a spouse. Since only sick individuals with a spouse can compensate the loss of household income through spousal labor supply, they might less often make the effort to go back to work than singles do, particularly if their labor market position is weak. Figure 8 in Appendix C compares labor participation and benefit receipt of sick people with and without a spouse of control and treatment groups. It suggests that indeed, the positive reform effect on labor participation is substantially larger for the sick individuals without a spouse. Figures for wages and benefit amounts lead to the same conclusions (not shown).

Table 9 in Appendix C presents summary statistics for sick people without a spouse. As before, the (small) differences between the treatment and control groups will be taken into account by our empirical strategy. Comparing with Table 1a, singles tend to be younger and more often female than sick people with a partner. They are also less likely to have a permanent contract at the time of reporting sick, which suggests it might be important to allow for heterogeneity by labor market conditions.

Table 6 presents the DiD estimates of the reform effects for sick people without a spouse, and reproduces the baseline estimates for sick people with a spouse from Table 2.<sup>13</sup> The estimates confirm that the reform increases the probability of working post-treatment by 1.2 pp more among sick people without a spouse than for sick individuals in couples. Compared to the effects on DI receipt, the effects on labor participation are almost twice as large for singles than for partnered individuals: 61% vs. 32%. Together with the earlier finding that spouses increase their labor participation in response to the reform (Table 2), this suggests that in couples, the response to the disability reform is shared by both partners: Spousal labor supply is a substitute for sick individuals' own labor supply. The reform effects for earnings and benefits are in line with this. For example, sick people without a spouse increase their earnings by 15.8% in response to the reform, whereas for sick people with a spouse the increase in earnings is only 7.6%.

As in Section 6, we consider the possibility that the reform effects depend on the time since the individual fell sick, see Figure 9 in Appendix C. The time patterns of the effects on labor participation are similar for sick people with and without a spouse, but substantially larger for sick people without a spouse, also in the long run. The time patterns of the effects on DI and UI receipt are also similar to those with and without a spouse – persistent and significant throughout the entire post-treatment period. Similar time patterns are found for the effects on wage and benefit amounts (not shown).

In Section 6.3 we analyzed heterogeneity in labor supply responses to the DI reform to better understand how couples make joint labor supply decisions. We conducted a heterogeneity analysis for singles and compare with individuals in couples in Table 7.<sup>14</sup> Like before, we focus on heterogeneity in terms of labor market position when falling sick, characterized by type of contract (top panel), earnings level (middle panel), or vacancy rate in the sector (bottom panel). First, the effects of the reform on the chances to receive disability benefits after the sickness period are negative and similar for individuals in couples and singles in all cases. The point

<sup>13</sup>Figure 9 in Appendix C presents the estimates of pre-treatment effects for sick individuals without a spouse for all outcomes, supporting the common trend assumption.

<sup>14</sup>As for the partnered sick individuals, we find similar effects for male and female singles (results not presented).

estimates tend to be somewhat larger for singles, but the differences with partnered individuals are not significant, even though individuals in couples and singles may also differ in other characteristics. The most interesting part in the table is the middle column. Individuals with a relatively strong labor market position (permanent contract, high earnings, or low vacancy rate sector) respond themselves, irrespective of whether there is a spouse or not. In particular, the responses for singles and non-singles with a permanent contract are remarkably similar, even though the two groups may differ in many other characteristics. This group has relatively good chances to go back to work and does not need to rely on a partner (if there is one). On the other hand, the sick individuals that more often struggle to go back to work (temporary workers, for example) respond much more if they are single than if they have a partner. It suggests that in these cases, it is easier for couples if the partner of the sick individual responds, whereas for singles this option does not exist, and the individual makes a larger effort to go back to work. The effects on UI receipt never differ significantly between sick individuals with and without a spouse.

Similar results are obtained for monthly earnings and benefits (Table 8). The main difference between sick individuals with and without a spouse is the response in earnings for those on a temporary contract or in the lower pre-sickness earnings groups: it is much larger for singles, who cannot compensate the loss in household income through spousal earnings. There is not much difference between the reform effects on labor participation in the sectors with lower and higher vacancy rates, suggesting the vacancy rate is not a strong indicator of the opportunities to go back to work.

Figures 5 to 7 in Appendix B present the dynamic effects by employment status, vacancy rate, and earnings quartile for all groups: sick people with a spouse, their spouses, and sick people without a spouse. They are in line with the main findings. In groups where spouses respond, sick individuals without spouse also respond, confirming that in couples both partners share the burden of a more stringent DI scheme. The estimated effects at individual event years are not always significant at the 5 percent level, however.

Table 6: Estimated effects of the WIA reform: Individuals with and without spouse

	Sick individual with a spouse	Sick individual without a spouse
DI receipt	-0.031*** (0.002)	-0.036*** (0.004)
Labor participation	0.010*** (0.003)	0.022*** (0.005)
UI receipt	0.008*** (0.001)	0.010*** (0.002)
ln DI	-0.203*** (0.018)	-0.246*** (0.027)
ln Wage	0.076*** (0.027)	0.158*** (0.038)
ln UI	0.056*** (0.009)	0.072*** (0.012)
ln Total individual income	-0.010 (0.023)	-0.008 (0.032)
Observations	8,431,218	4,425,372
Individuals	55,106	28,924

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period and exclude data for the first two years of the post-treatment period.

Table 7: Estimated effects of the WIA reform on DI receipt, labor participation and UI receipt of sick people with and without a spouse by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	DI receipt		Labor participation		UI receipt	
	Sick people with a spouse	Sick people without a spouse	Sick people with a spouse	Sick people without a spouse	Sick people with a spouse	Sick people without a spouse
On permanent contract	-0.027*** (0.002)	-0.022*** (0.004)	0.016*** (0.004)	0.015** (0.006)	0.004*** (0.001)	0.007*** (0.002)
On temporary contract	-0.031*** (0.009)	-0.036*** (0.009)	-0.015 (0.012)	0.022* (0.011)	0.013*** (0.004)	0.015*** (0.004)
Unemployed	-0.041*** (0.008)	-0.055*** (0.010)	-0.014 (0.009)	0.000 (0.011)	0.021*** (0.005)	0.016*** (0.005)
Earnings in 4th quartile	-0.037*** (0.004)	-0.037*** (0.007)	0.013* (0.007)	-0.000 (0.009)	0.007*** (0.002)	0.012*** (0.003)
Earnings in 3rd quartile	-0.036*** (0.005)	-0.034*** (0.007)	0.004 (0.006)	0.022** (0.009)	0.007*** (0.002)	0.006* (0.003)
Earnings in 2nd quartile	-0.024*** (0.005)	-0.040*** (0.008)	0.010 (0.007)	0.031*** (0.009)	0.008*** (0.002)	0.015*** (0.004)
Earnings in 1st quartile	-0.025*** (0.006)	-0.033*** (0.009)	0.006 (0.007)	0.028*** (0.010)	0.011*** (0.003)	0.007* (0.004)
Vacancy rate above the mean	-0.022*** (0.003)	-0.031*** (0.005)	0.010** (0.005)	0.020*** (0.007)	0.008*** (0.002)	0.011*** (0.002)
Vacancy rate below the mean	-0.039*** (0.004)	-0.040*** (0.006)	0.008* (0.005)	0.023*** (0.007)	0.008*** (0.002)	0.010*** (0.002)

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

Table 8: Estimated effects of the WIA reform on DI benefits, wages, UI benefits and total income of sick people with and without a spouse by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	In DI		In Wage		In UI		In Total individual income	
	Sick people with a spouse	Sick people without a spouse						
On permanent contract	-0.183*** (0.018)	-0.142*** (0.030)	0.130*** (0.031)	0.112** (0.050)	0.027*** (0.009)	0.050*** (0.014)	0.029 (0.027)	0.060 (0.041)
On temporary contract	-0.196*** (0.067)	-0.248*** (0.065)	-0.126 (0.089)	0.182** (0.090)	0.092*** (0.030)	0.105*** (0.026)	-0.180** (0.075)	-0.046 (0.075)
Unemployed	-0.265*** (0.059)	-0.378*** (0.073)	-0.126* (0.070)	-0.015 (0.083)	0.148* (0.033)	0.118*** (0.037)	-0.145** (0.062)	-0.212*** (0.069)
Earnings in 4th quartile	-0.260*** (0.033)	-0.241 *** (0.050)	0.121** (0.057)	0.011 (0.074)	0.051*** (0.016)	0.086*** (0.023)	0.015 (0.050)	-0.026 (0.061)
Earnings in 3rd quartile	-0.232*** (0.035)	-0.226*** (0.053)	0.030 (0.052)	0.176** (0.071)	0.048*** (0.015)	0.043* (0.024)	-0.031 (0.043)	0.040 (0.054)
Earnings in 2nd quartile	-0.152*** (0.036)	-0.275*** (0.056)	0.076 (0.052)	0.241*** (0.072)	0.057*** (0.017)	0.109*** (0.026)	0.017 (0.041)	0.064 (0.056)
Earnings in 1st quartile	-0.164*** (0.041)	-0.227*** (0.061)	0.039 (0.053)	0.220** (0.074)	0.073*** (0.024)	0.050* (0.023)	-0.056 (0.051)	-0.051 (0.069)
Vacancy rate above the mean	-0.145*** (0.025)	-0.209*** (0.040)	0.084** (0.040)	0.146*** (0.053)	0.058*** (0.013)	0.076*** (0.017)	0.037 (0.033)	-0.007 (0.044)
Vacancy rate below the mean	-0.258*** (0.026)	-0.269*** (0.040)	0.064* (0.039)	0.169*** (0.056)	0.060*** (0.013)	0.072*** (0.019)	-0.048 (0.034)	0.016 (0.046)

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## 8 Checking the identifying assumptions

### Is the pre-treatment time trend common to control and treatment groups?

Our main identifying assumption is that, conditional on observables, control and treatment groups share the same time trend in the potential outcome variables before and after individuals report sick and face the reform incentives or not. The assumption is testable during the pre-treatment period. Figure 1 already suggested that control and treatment groups, both for sick individuals and their spouses, share very similar time trends until individuals fall sick, supporting this identifying assumption. For a formal test, we use equation (2). Statistically insignificant estimates on the treatment and annual dummy interactions during the pre-treatment period provide evidence supporting the assumption. Year  $-1$  is chosen as the base for comparison. Figure 2 plots the estimates for sick individuals (left hand panel) and their spouses (right hand panel). For both groups and all outcomes, the estimates are insignificant throughout the pre-treatment period. They are also jointly insignificant, with p-values of at least 70%. The estimates are also (individually and jointly) insignificant for wages and benefit amounts, and in all sub-group analyses of heterogenous treatment effects (available upon request).

### Placebo test: Is a treatment effect absent in a non-reform year?

The effects we find could be not due to the reform but due to, for example, some seasonal effect that leads to different changes in labor market position for those who fell sick before and after January 1 2004 (the control and treatment groups, respectively). To confirm that we effects we find are indeed due to the reform, we performed the same DiD estimation (Equation 1) comparing the groups who reported sick one year later (last quarter of 2004 and first quarter of 2005). Both groups fall under the new WIA regime so there should not be any reform effects. Table 10 in Appendix D shows that, indeed, for both the sick individuals and their spouses, estimated treatment effects are close to 0 for all outcomes, and insignificant at (at least) the 10 percent level.

### Are the results robust to a regression discontinuity approach?

An alternative identification strategy is a regression discontinuity (RD) approach, using the date of falling sick as the running variable (since the reform applies to those who reported sick as of January 1, 2004). In Appendix E we present the results. Both identification strategies lead to the same qualitative conclusions for all outcomes and to similar relative sizes of the effects across sick individuals with and without a spouse and for spouses. On the other hand, the RD estimates are typically much larger than the DiD estimates. A possible explanation is that individuals who report sick just before and just after January 1 are different, due to the Christmas holidays. For example, workers in specific sectors or professions may continue working during the last weeks of the calendar year, whereas others do not. If the difference affects levels but not trends, this is accounted for in the DiD estimates but not in the RD estimates.

### Do individuals self-select into the old or new disability scheme?

Reporting sick before or after January 1 2004 determines eligibility for either WAO or WIA, implying that individuals with adverse health shocks in 2003 might select themselves into the WAO or WIA scheme from the time the reform is announced. In particular, the government presented a sketch of its reform plans on 15 September 2003, announcing that the sickness period would be extended from one to two years and that a stricter DI law would be introduced for

individuals reporting sick as of 1 January 2004. The transitional WAO reform was announced on 12 March 2004, and details of the WIA reform were announced on 18 August 2004. Following the first announcement in September 2003, individuals could report sick during the last quarter of 2003 instead of after 1 January 2004 to enter the more lenient WAO scheme instead of WIA. In principle, they also might want to postpone their sickness claim until January 2004, to get an additional year of sickness benefits. This seems unlikely since the sickness benefit falls from 100% of the former wage in the first year to 70% in the second year, generally making income while on sickness benefits lower than if on DI or UI (cf. Section 2). If individuals strategically choose the disability regime, our results could be biased.

We argue that such self-selection is unlikely. Figure 3 shows how many individuals reported sick in the last quarter of 2003 and first quarter of 2004. The distribution is fairly uniform and does not suggest any particular pattern. It certainly does not suggest that many individuals report sick in the last quarter of 2003 instead of early 2004. On the contrary, if anything, there are more sick reports in January 2004, after the stricter WIA scheme was introduced. The relatively low number of workers reporting sick in December 2003 is probably due to a seasonal employment pattern in absence from work, implying that few people report sick during the Christmas and New Year holidays. This is confirmed by the numbers reporting sick one year after the reform, also presented in Figure 3. This distribution is very similar to that the year before when the reform was introduced.

In addition, self-selection would be plausible among people with mild impairments only, who would be able to manipulate the timing of their sick reporting. However, both pre- and post-reform, the same Gatekeeper protocol was in place, according to which after 6 weeks of sickness a first reintegration plan has to be submitted to the Employee Insurance Agency by the employer. Due to this formal screening, mild sickness cases tend to be denied sickness benefits already at this stage.

Finally, if some individuals manage to select themselves into one of the two DI schemes, they would probably do this around 1 January 2004 when the WIA reform came into effect. If we exclude individuals who reported sick within two weeks before or after this date, our DiD results in the heterogeneity analysis remain very similar – see Appendix F.

### **Do couples separate due to the reform?**

We study the labor supply responses of couples to the DI reform who started cohabiting before reporting sick. Cohabitation can end during the post-treatment period due to the reform or other reasons. In this case the estimated treatment effect may not only reflect the labor supply responses to the DI reform. In the sample, we find no statistical difference between the fractions of couples whose cohabitation ends in the treatment and control groups during the post-treatment period, suggesting that the reform has no effect on cohabitation status – see Appendix G.

### **Can compositional differences drive heterogeneity in the reform effects?**

In Section 6.3 we documented clear reform effects of spousal labor supply responses for individuals reporting sick with a weak labor market position. A threat to our identification strategy might be that the two groups, e.g. permanently and temporarily employed, could differ in other observable and unobservable characteristics, correlated with employment status. For example, individuals with a temporary contract tend to be younger than individuals with a permanent contract. For younger individuals, sickness may represent a larger or more unexpected shock, they might have limited eligibility for UI, and they might have accumulated less wealth to smooth the negative income shock, all of which may lead to a stronger spousal response among

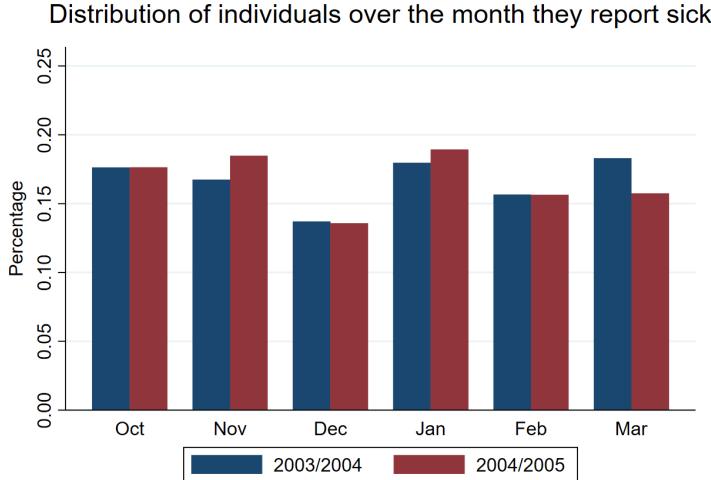


Figure 3: Distribution of the number of individuals reporting sick, among those who reported sick in the last quarter of 2003 and first quarter of 2004 and participated, respectively, in the WAO and WIA, and those who reported sick in the last quarter of 2004 and first quarter of 2005 and participated in the WIA.

younger couples. To address this, we consider gender and age as main variables driving compositional differences across labor market groups. We weigh individuals across labor market groups to have similar distributions of gender and age of the spouse across these groups. We then estimate equation (1) within each labor market group using the re-weighted sample of that group. In other words, we control for compositional differences between control and treatment group, but also between the groups with different labor market status when reporting sick. In Appendix H we show that the estimated treatment effects in heterogenous labor market groups are robust to these compositional differences across these groups, suggesting that spousal responses indeed stem from weak labor market conditions.

## 9 Conclusion

We have analyzed the labor supply and earnings responses of individuals who reported sick and their spouses to a major reform of the Dutch disability insurance (DI) system that introduced stricter eligibility criteria for DI and stronger employer and employee incentives for work resumption. An advantage compared to earlier studies is that we use unique administrative data that include everyone who spent more than three months on sickness benefits, not only the individuals who entered disability after the sickness benefit period expired. We focus on the added worker effects on spouses: Since couples can pool income risk, spousal labor supply can be an important self-insurance mechanism to counterbalance the loss of income due to the reform. Based on a difference-in-differences identification strategy, we find clear evidence of an added worker effect for spouses of workers who report sick from a weak labor market position where work resumption is difficult. Compared to the reform effect on disability benefit receipt and work resumption of the sick individuals themselves, the effect on the spouse's labor participation is substantial (about one sixth and one half, respectively). This finding is notable given that an earlier major DI reform (implemented in 1993) had no significant effect on spousal labor supply (Borghans et al., 2014). It implies that for a complete evaluation of the DI reform and

its effects on labor participation as well as adequacy of household income, it is important to consider spill-over effects on spouses.

The effect of the reform on spousal labor supply depends on the type of the employment contract of the sick individual when falling sick. People who had a permanent contract at the time they fell sick increased labor market participation by 1.6 pp due to the reform, while their spouses did not respond. On the other hand, people who had a temporary contract when they fell sick did not increase labor participation because of the reform, but their spouses increased labor participation by 2.5 pp. Furthermore, the spousal response is persistent during the ten years following the start of sickness. Overall, the response at the couple level is sizable regardless in all cases, driven by either the response the sick partners, or the spouses. The effect of the reform also depends on the vacancy rate in the sector where the sick individual was working: if this vacancy rate was above the mean, they increased labor participation by 1 pp themselves while their spouses did not respond, but if the sectoral vacancy rate was below the mean, only the spouses increased labor participation, by a significant 0.9 pp. Finally, spouses increased labor participation more often if their sick partners were low wage earners. All these findings support the hypothesis that partners substitute for each other's labor force participation and spouses respond more often if the labor market position of the sick individual is weaker. Comparing individuals reporting sick with and without partner provide additional evidence for this hypothesis.

Most of the earlier estimates of the added worker effect are small. Our findings add to the few recent studies that find economically meaningful added worker effects (Section 1). On average, the extensive margin added worker effect is 16%, as spouses' labor participation increases by 0.5 pp in response to the 3.1 pp drop in DI receipt for sick partners due to the reform. The extensive margin added worker effect attains 81% for sick partners with temporary contracts and it attains 52% and 23% respectively for sick partners with low earnings and for sick partners working in a low vacancy sector. There are several reasons why the 2006 Dutch DI reform did lead to a substantial added worker effect. First, the reform led to a permanent reduction of the income of the affected individual. In line with this, we find persistent responses of both the sick individuals and their spouses in the ten years following sickness (Figure 2). Second, the reform could not be anticipated so that couples could not adjust their consumption and labor supply before the reform took place. Third, as the DI reform limited DI entitlement, social protection has become weaker and the need for households' self-insurance increased. These arguments apply particularly if the chances to resume work for the individual who fell sick are small, e.g. since the sick individual had a temporary work contract or was unemployed.

## Conflict of interest statement

There is no conflict of interest for any of the four authors of the manuscript. Please also see enclosed for each author the conflict of interest statements.

## Data availability statement

Results are based on calculations by the authors using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information: [microdata@cbs.nl](mailto:microdata@cbs.nl).

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## Appendix A: Timeline of changes in the Dutch DI scheme

1 July 1967	Disability Insurance Act (WAO)	<ul style="list-style-type: none"> <li>• Minimum disability grade for DI eligibility: 15%.</li> <li>• Duration of SI: 1 year.</li> </ul>
1 August 1993	Reduction in DI Benefit Use Act (TBA)	<ul style="list-style-type: none"> <li>• Major amendments, such as stricter entitlement criteria, financial incentive to resume working, reexaminations.</li> </ul>
1 March 1996	Wage Compensation during SI (Wulbz)	<ul style="list-style-type: none"> <li>• Employers are obliged to compensate at least 70% of pre-sickness wage during SI.</li> </ul>
1 January 1998	Experience Rating Act (Pemba)	<ul style="list-style-type: none"> <li>• DI is financed by premiums that are experience rated for the last 5 years.</li> <li>• Applied only to workers with permanent work contracts.</li> </ul>
1 April 2002	Gatekeeper Protocol (Wvp)	<ul style="list-style-type: none"> <li>• New reintegration obligations for employers and employees are introduced in the SI scheme.</li> </ul>
1 October 2004	Transitional WAO (aSB)	<ul style="list-style-type: none"> <li>• A broader definition of what work the DI applicant can still do is adapted.</li> </ul>
1 January 2006	Work and Income According to Labour Capacity Act (WIA)	<ul style="list-style-type: none"> <li>• Applies to workers who reported sick since 1 January 2004.</li> <li>• Minimum disability grade for DI eligibility: 35%.</li> <li>• Duration of SI: 2 years.</li> <li>• Wage compensation and Gatekeeper Protocol are exercised for an additional year in SI compared to the (transitional) WAO.</li> <li>• Experience Rating Act is extended from 5 to 10 years and restricted to temporarily or partially disabled workers and abolished for permanently and fully disabled workers.</li> <li>• A work resumption program with financial incentives to utilize remaining work capacity is introduced for the partially disabled.</li> <li>• A generous full benefit scheme is introduced for the permanently and fully disabled.</li> </ul>
1 January 2013	Extended Experience Rating Act (Bezava)	<ul style="list-style-type: none"> <li>• Experience Rating Act is extended to workers with temporary work contracts.</li> </ul>

Figure 4: Timeline of changes in the Dutch DI scheme.

## Appendix B: Dynamic heterogenous effects

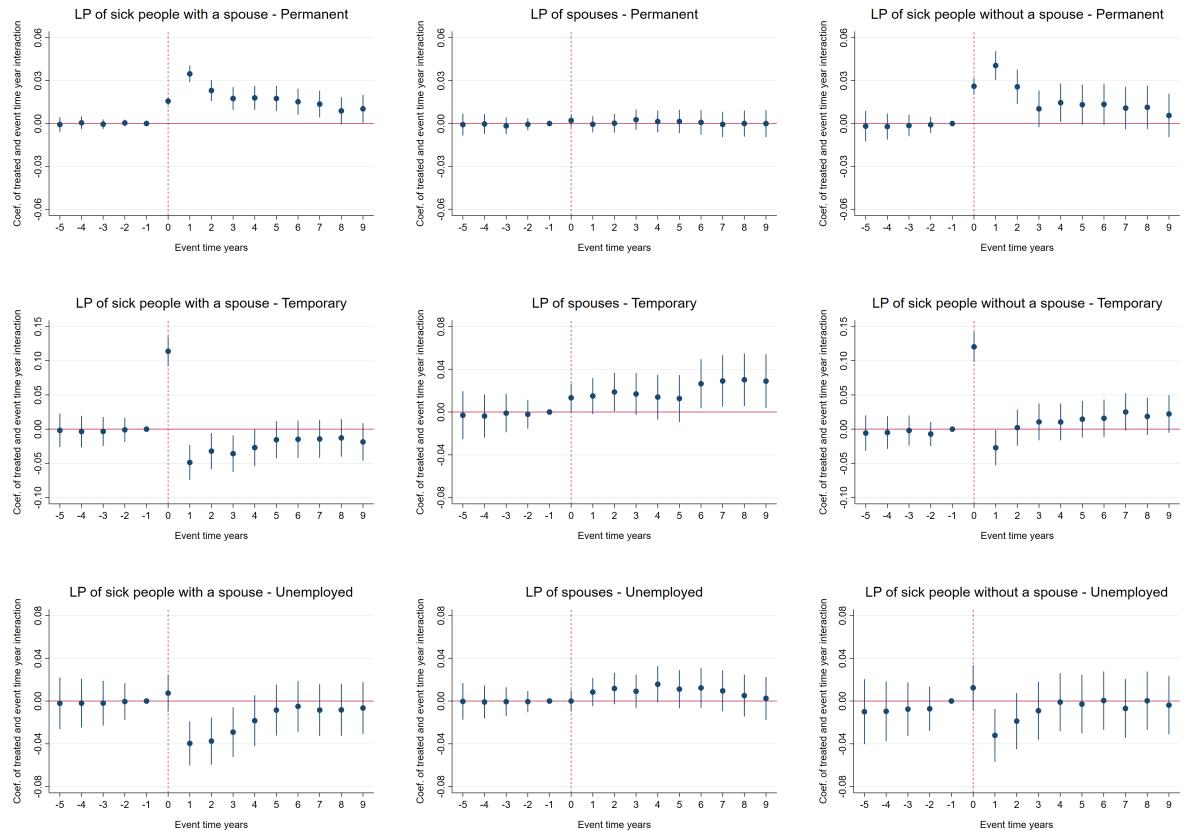


Figure 5: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals for sick individuals, their spouses, and sick individuals without spouses, by labor market status when reporting sick. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

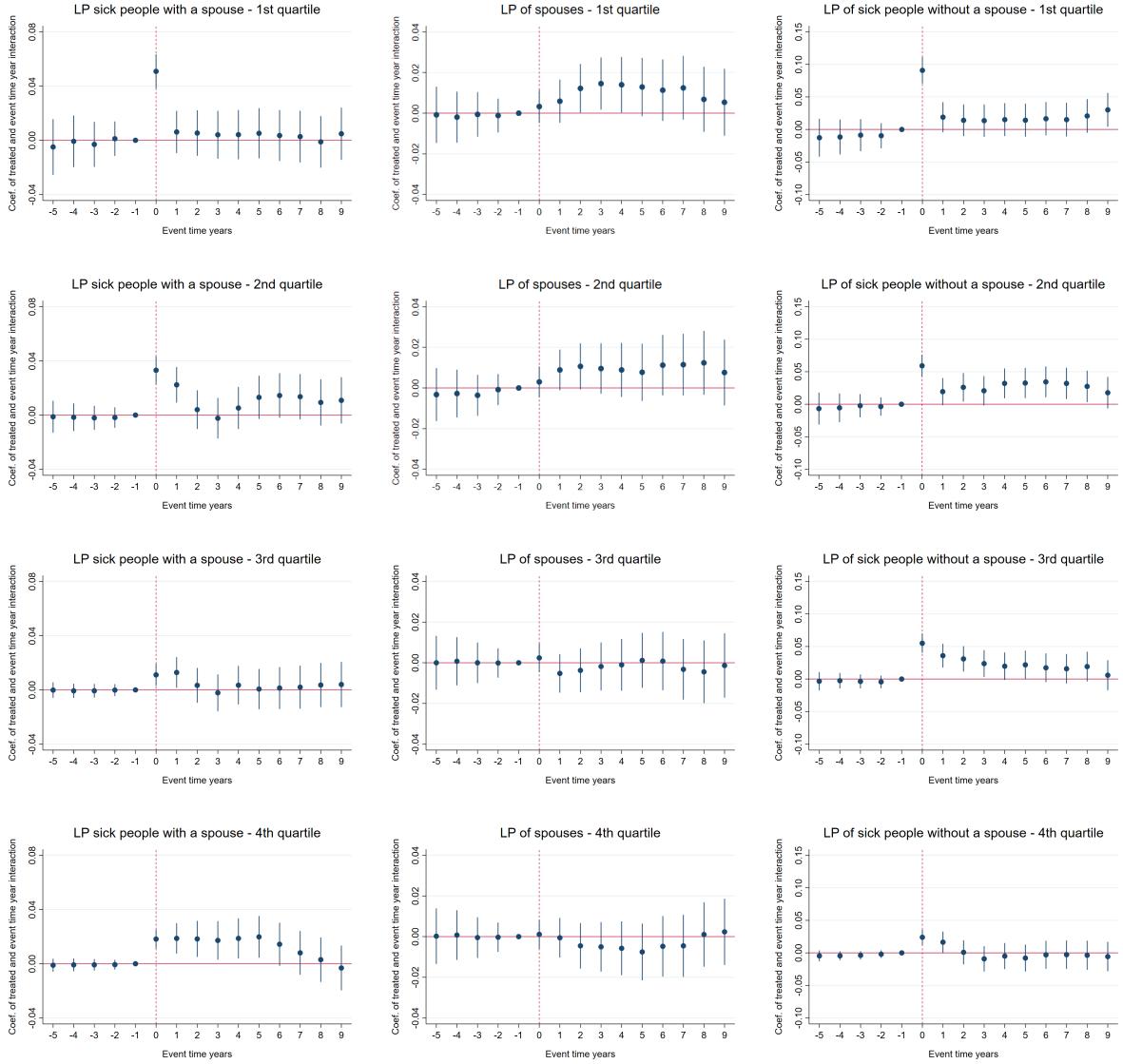


Figure 6: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals for sick individuals, their spouses, and sick individuals without spouses, by earnings quartile. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

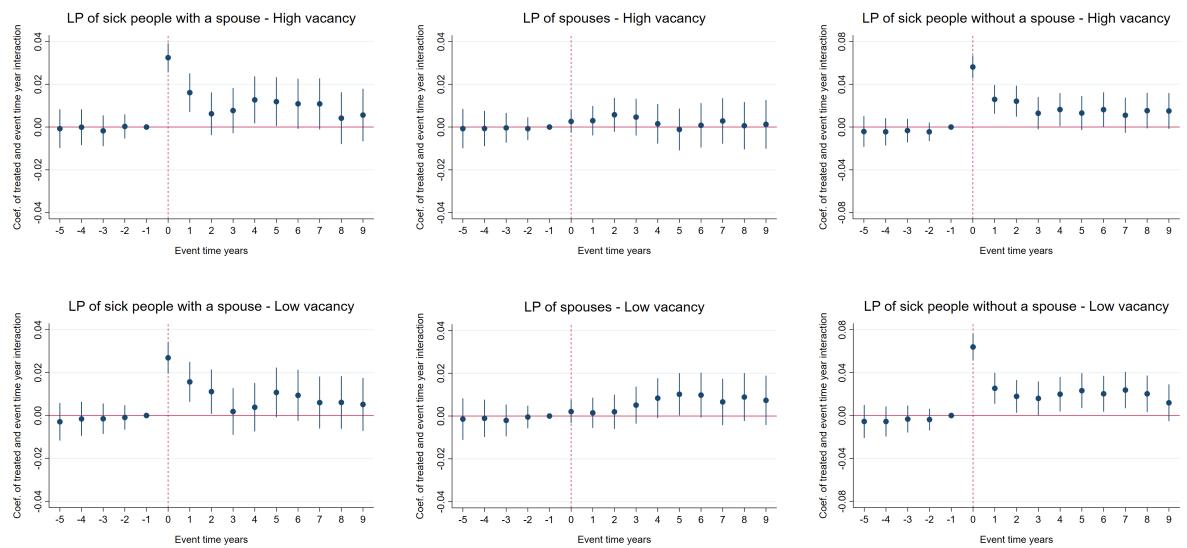


Figure 7: Estimated treatment effects in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals for sick individuals, their spouses, and sick individuals without spouses, by sectoral vacancy rate above (high) or below (low) the average vacancy rate in all sectors. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

## **Appendix C: Comparing with individuals without a spouse who report sick**

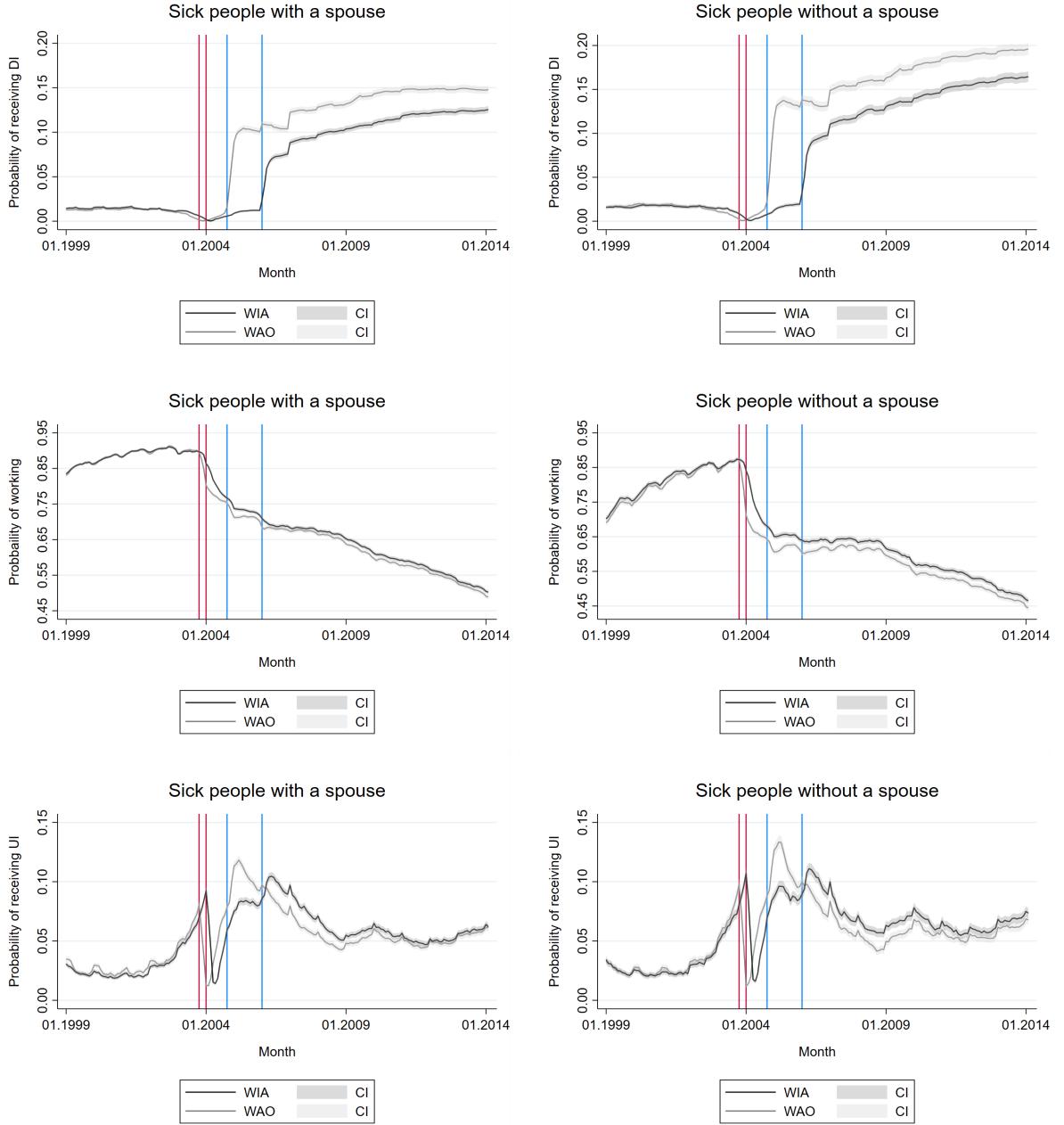


Figure 8: Probability of DI receipt, working, and UI receipt for control and treatment groups over calendar months: for sick individuals with a spouse (left panel reproducing the left panel of Figure 1) and without a spouse (right panel). Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the WAO and WIA groups, respectively. Blue lines correspond to 1 October 2004 and 1 January 2006 for the WAO and WIA groups, respectively.

Table 9: Sample means and balancing tests of background characteristics and outcome in control and treatment groups before and after sickness for sick individuals without a partner

	Before		After		
	WAO group	WIA group	Dif. WAO	WAO group	WIA group
	(1)	(2)	(3)	(4)	(5)
<b>A. Background characteristics</b>					
Age	36.855	37.365	0.501***		
Female	0.450	0.454	0.004		
Permanent contract	0.547	0.596	0.049***		
Temporary contract	0.220	0.184	-0.036***		
Unemployed	0.233	0.220	-0.013***		
<b>B. Labor market outcomes</b>					
DI (possibly UI) receipt				0.170	0.136
Labor participation	0.807	0.822	0.015***	0.561	0.579
UI (no DI) receipt	0.034	0.035	0.001	0.061	0.070
DI (and possibly UI) per month				224.508	195.771
Wage per month	1,462.196	1,530.015	67.819*** <sup>1</sup>	391.216	1,454.990
UI (excl. DI) per month	41.925	44.290	-3.022*	77.395	70.084
Observations	849,300	900,060		1,358,880	1,440,096
Individuals	14,155	15,001		14,155	15,001

Notes: 1. “Before”: period before individuals fall sick (January 1999 - October 2003 for individuals who fell sick in November 2003; January 1999 - January 2004 for individuals who fell sick in February 2004). “After”: period after individuals fell sick excluding the first two years (November 2005 - January 2014 for individuals who fell sick in November 2003; February 2006 - January 2014 for individuals who fell sick in February 2004). 2. Age is at the time individuals fall sick. “Permanent contract”, “temporary contract”, and “unemployed” refer to labor market status of individuals when they fell sick. 3. Columns 1, 2, 4 and 5 present means in control (WAO) and treatment (WIA) group before and after start of sickness. Columns 3 and 6 present differences between individuals insured under WIA and WAO – the estimated coefficient from the regression of the characteristic or outcome as the dependent variable, and an indicator of participation in WIA as the explanatory variable. Standard errors clustered at the individual level.

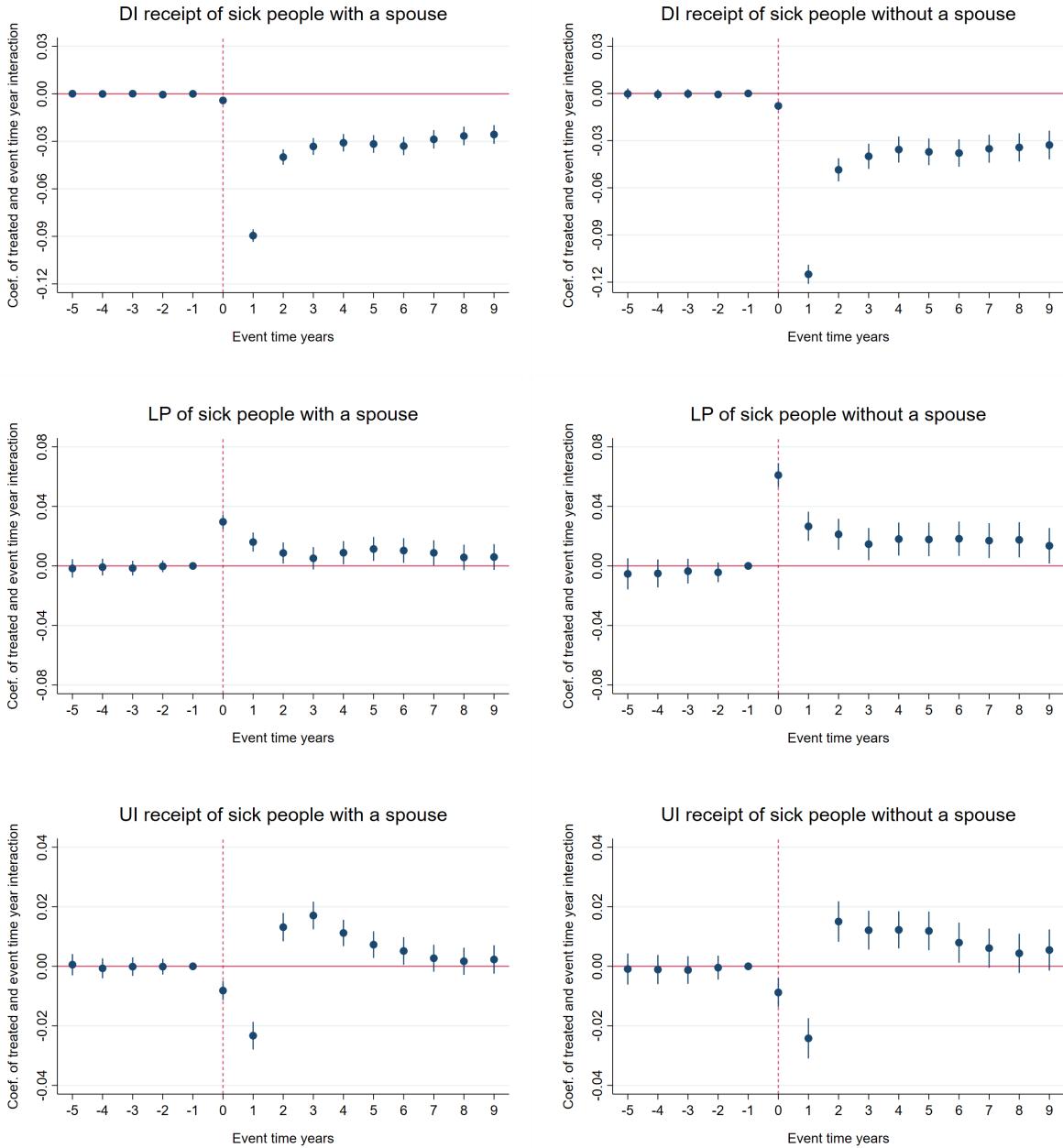


Figure 9: Estimated treatment effects in each of the first ten years after falling sick, with 95 percent confidence intervals. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.

## Appendix D: Placebo test

Table 10: Estimated effects in a non-reform year: Sick individuals and their spouses

	Sick individual	Spouse
DI receipt	0.000 (0.002)	0.000 (0.002)
Labor participation	0.003 (0.004)	0.002 (0.003)
UI receipt	0.010 (0.001)	-0.000 (0.001)
ln DI	0.006 (0.018)	0.003 (0.013)
ln Wage	0.033 (0.029)	0.021 (0.025)
ln UI	0.006 (0.010)	-0.005 (0.007)
ln Total individual income	0.042 (0.026)	0.015 (0.024)
ln Total household income	0.031 (0.021)	
Observations	7,480,935	
Individuals	48,895	

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## Appendix E: Regression Discontinuity instead of Difference-in-Differences

Our DiD estimates of the effect of the WIA reform rely on the assumption that trends of the outcome variable over event time would have been the same for the treatment and control groups had the reform not been implemented. Although it is not possible to directly test this assumption, we provided evidence that trends are parallel in the pre-treatment period. Here we argue that the fact that we find a significant effect of the reform does not depend on the specific identifying assumption we made. We consider an alternative identification strategy that relies on different identifying assumptions, and the results confirm the results based on the DiD method.

We exploit the date at which the WIA reform came into effect as a source of exogenous variation in treatment status. The assignment to the treatment or control group is a deterministic step-function of the date at which people reported sick – people who reported sick right before 1 January 2004 are insured under the WAO scheme, while people who reported sick right after this “cut-off” date are insured under WIA. We rely on a sharp regression discontinuity (RD) design to estimate the effect of the reform. In particular, the discontinuous jump at the cut-off identifies the treatment effect of interest which can be formalized as

$$\lim_{x \downarrow c} \mathbb{E}[Y_i | X_i = x] - \lim_{x \uparrow c} \mathbb{E}[Y_i | X_i = x] \quad (3)$$

where  $X_i$  is the date at which people report sick and  $c$  is the cut-off point of 1 January 2004. The treatment effect is estimated using a triangular kernel and a MSE-optimal bandwidth selector (see [Calonico et al., 2014](#)). We use a robust variance estimator clustered at the individual level in order to account for the correlation of the error terms across calendar months for the same individual. We consider the same time horizon as with the DiD estimates – the period after treatment but excluding the first two years. We pool all monthly observations of the post-treatment period excluding the first 24 months, implying that we have 96 observations for each individual. We do not account for individual fixed effects but this should not result in biased estimates since the distance from the cut-off date is assumed to be random for individuals who report sick close to 1 January 2004.

The sharp RD design relies on two main assumptions ([Imbens and Lemieux, 2008](#)). The first assumption requires a sharp discontinuity in treatment. This assumption holds in our setting by design of the reform, since all individuals  $i$  for which  $X_i \geq c$  are in the treatment group (WIA regime) and all individuals  $i$  for which  $X_i < c$  are in the control group (WAO regime).

The second assumption requires continuity in potential outcomes as a function of the assignment variable around the cut-off point. This implies that had the reform not been implemented, the outcome variables should not discontinuously jump at the cut-off point. In other words, “all other factors” driving the outcome variables must be continuous at the cut-off point (see, e.g., [Hahn et al., 2001](#)). Although this assumption cannot be tested directly, relevant variables can be checked for whether they change significantly at the cut-off. We consider contract type at the time of reporting sick as a most relevant variable. We consider dummies for having a permanent contract, temporary contract and being unemployed as outcome variables, and check if they exhibit discontinuity at the cut-off. For sick people with a spouse, we find no significant change at the cut-off in any of the three outcomes. For sick people without a spouse, however, the RD estimate of the treatment effect on being unemployed is -0.088 with a standard error of 0.028. Therefore, we treat the RD estimates of the reform effects as suggestive rather than conclusive, at least for sick people without a spouse.

[Figure 10](#) provides graphical evidence for labor participation and benefit receipt. In the figure we distinguish among sick people with a spouse, spouses of sick individuals, and sick

individuals without a spouse. For each sample, the figure shows local linear fits for outcome with symmetric bandwidth thirty days around the cut-off date. The figure shows clear discontinuities at the cut-off point, in the expected direction. Furthermore, the relative size of the jumps are in line with the DiD estimates presented in Tables 2 and 6. For example, for labor participation, sick people without a spouse show the largest effect, followed by sick people with a spouse and by the spouses of sick individuals.

Table 11 presents estimated average treatment effects at the cut-off. Both the RD and DiD estimators provide evidence of a positive and significant effect of the reform on the employment probability of spouses and sick individuals with or without spouse. The RD estimates, however, are larger than the DiD estimates. A possible explanation is the fact that RD only identifies the average treatment effect at the cut-off point, that is, for a specific group of people who report sick around 1 January. These people might differ from those who report sick in other months of the year. Overall, both identification strategies provide evidence that sick people in couples rely on the labor supply of their spouses to counterbalance the effect of the DI reform. This is confirmed by the finding that, due to the reform, sick individuals without a spouse increase their labor participation more as they are not able to compensate through spousal labor supply.

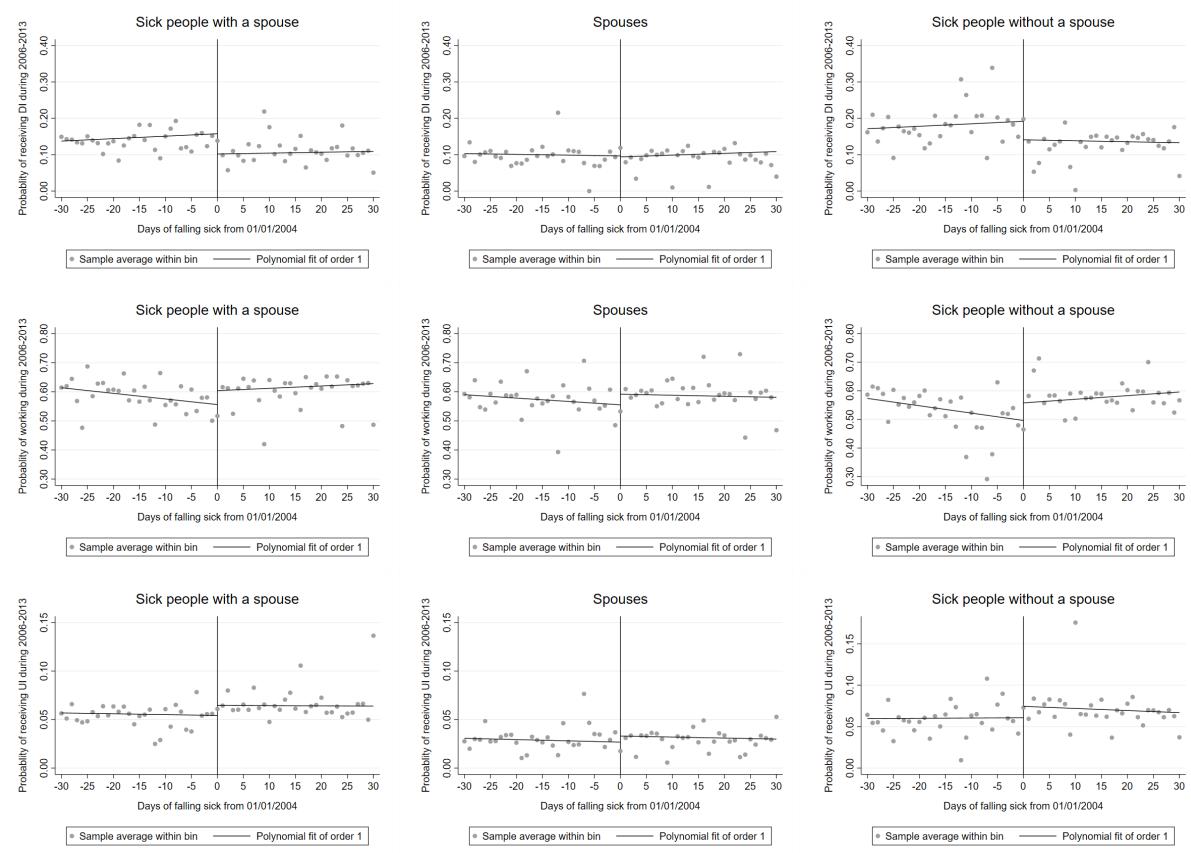


Figure 10: Local linear fit on the two sides of the cut-off. Standard errors are clustered at the individual level. All subfigures exclude data for the first two years after reporting sick.

Table 11: Sharp RD estimate of the effect of the reform on the labor participation of sick individuals and their spouses and of sick individuals without spouse

	Sick individual with a spouse	Spouse	Sick individual without a spouse
DI receipt	-0.054*** (0.012)	-0.001 (0.012)	-0.051*** (0.017)
Labor participation	0.048*** (0.017)	0.038** (0.019)	0.059*** (0.022)
UI receipt	0.010** (0.004)	0.004 (0.003)	0.014*** (0.006)
ln DI	-0.382*** (0.089)	-0.013 (0.085)	-0.374*** (0.117)
ln Wage	0.375*** (0.131)	0.316** (0.156)	0.434** (0.176)
ln UI	0.076** (0.030)	0.025 (0.021)	0.103** (0.043)
ln Total individual income	0.123 (0.096)	0.327*** (0.139)	0.170 (0.135)
ln Total household income	0.110 (0.089)		

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5, and 10 percent, respectively. The estimates are obtained using a triangular Kernel and an MSE-optimal bandwidth selector. Standard errors are clustered at the individual level. The regressions are based on post-treatment data excluding the first two years. Effective number of observations and individuals used in the estimations depend on the bandwidth. For example, 1,354,272, 1,274,784 and 835,392 observations for 14,107, 13,279 and 8,702 individuals are used when the bandwidths (days) are 25.9, 24.3 and 29.0 in the regressions of labor participation of sick people with a spouse, spouses and sick people without a spouse, respectively.

## Appendix F: Do individuals self-select into the old or new disability scheme?

Table 12: Estimated effects of the WIA reform on labor participation excluding individuals who reported sick around the reform date: Sick individuals and their spouses

	Sick individual	Spouse
On permanent contract	0.016*** (0.004)	0.002 (0.004)
On temporary contract	-0.015 (0.012)	0.024** (0.010)
Unemployed	-0.017* (0.010)	0.006 (0.008)
Earnings in 4th quartile	0.014* (0.007)	-0.004 (0.007)
Earnings in 3rd quartile	0.003 (0.007)	-0.003 (0.006)
Earnings in 2nd quartile	0.008 (0.007)	0.010 (0.007)
Earnings in 1st quartile	0.005 (0.008)	0.013* (0.007)
Vacancy rate above the mean	0.010* (0.005)	-0.001 (0.005)
Vacancy rate below the mean	0.007 (0.005)	0.010** (0.005)

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## Appendix G: Do couples dissolve their cohabitation due to the reform?

We studied the labor supply responses of couples to the DI reform who started cohabiting before reporting sick. Couples can dissolve their cohabitation during post-treatment due to the reform or other reasons. This may confound the estimated reform effects. Here we check to which extent the reform affected cohabitation status during post-treatment.

The left panel of Figure 11 presents the probability that couples end their cohabitation during post-treatment. The probability is small and shows a decreasing time trend that is common to control and treatment groups. The confidence intervals for the two groups overlap which could suggest that the reform has no statistically significant effect on cohabitation status. These figures are in line with Table 1b which showed that couples in both the treatment and control groups cohabit for about 8 years on average during the the 10-year period of post-treatment.

To test whether the reform affected cohabitation status, we rely on a sharp RD design as in Appendix E. In particular, we exploit the date at which the reform came into effect as a source of exogenous variation in treatment status, and analyze whether sick-listed workers insured under the WAO and WIA differ in their cohabitation status during the ten years after reporting sick. The right panel of Figure 11 provides graphical evidence. It shows local linear fits for the probability that cohabitation ends with symmetric bandwidth thirty days around the cut-off date. The figure shows no discontinuity at the cut-off. The RD estimate (standard error in parenthesis) of the reform effect is 0.000 (0.000) and is statistically insignificant at the 10 percent level.<sup>15</sup> This shows that the reform did not cause couples to dissolve their cohabitation.

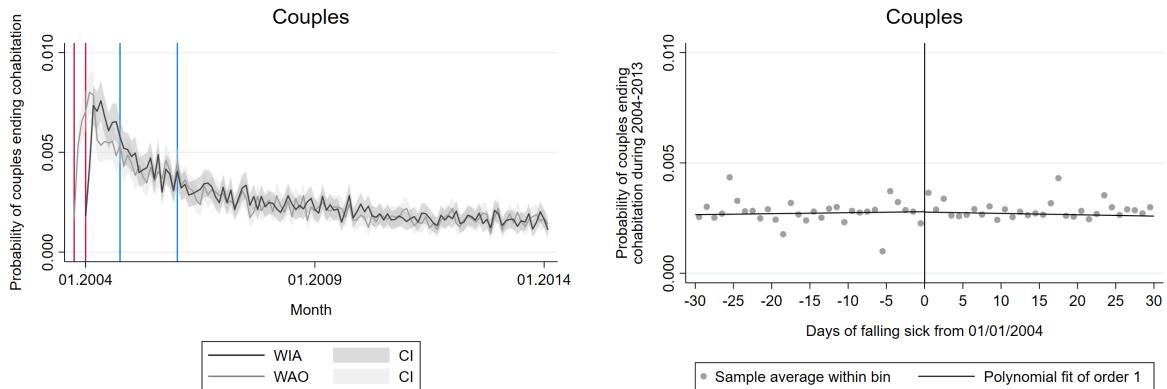


Figure 11: Left panel: Probability of couples ending cohabitation after one spouse falls sick. Vertical lines mark the first instance sick partners could become entitled to the sickness and disability benefits in the WAO and WIA schemes. Red lines correspond to 1 October 2003 and 1 January 2004 for the WAO and WIA groups, respectively. Blue lines correspond to 1 October 2004 and 1 January 2006 for the WAO and WIA groups, respectively. Right panel: Local linear fit on the two sides of the cut-off. Standard errors are clustered at the individual level. The figure uses 1,996,200 observations for 16,635 couples. The figure uses data for all available years after reporting sick.

<sup>15</sup>The RD estimation uses the MSE-optimal bandwidth and data for all available years after reporting sick which includes 1,398,000 observations for 11,650 couples.

## Appendix H: Can compositional differences account for the heterogeneous reform effects?

In Section 6.3 we showed that the added worker effect is a more powerful insurance mechanism when the labor market position of the sick individual affected by the reform is weak. In Section 7, our analysis of the heterogeneous reform effects among the sick individuals without spouses confirmed that in couples both partners share the burden of a more stringent DI scheme. Our estimated treatment effects across the labor market groups could in principle be driven by compositional differences across the groups. We consider gender and age as main variables driving compositional differences across labor market groups. We weight individuals across labor market groups to have similar distributions of gender and age of the spouse across these groups. With respect to employment status, for example, we take the mean age of spouses of sick individuals who have a temporary contract as reference, and weight spouses of sick individuals who are with a permanent contract or unemployed so that the three groups share similar distributions of age. We then estimate equation (1) within each labor market group using the re-weighted sample of that group.

The left most columns of Table 13 presents the fraction of women and average age among spouses of sick individuals in heterogeneous labor market groups in our study sample. Across the labor market groups, while differences in the average age of spouses are small, there is considerable variation in the fraction of female spouses. The right most two columns of Table 13 show the mean gender and age after re-weighting. Table 14 presents estimation results for labor participation of spouses. The left column reproduces estimates from Table 3 and the right column presents new results based on re-weighted samples. The table shows minor differences suggesting that differences in gender and age are not main drivers of the observed differences in responses across labor market groups.

Table 13: Sample means of gender and age for spouses by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate

	Original sample		Re-weighted sample	
	Female	Age	Female	Age
On permanent contract	0.671	42.879	0.537	39.069
On temporary contract	0.542	39.131	0.540	39.301
Unemployed	0.451	42.801	0.542	39.091
Earnings in 4th quartile	0.893	45.144	0.504	40.253
Earnings in 3rd quartile	0.817	42.141	0.509	40.243
Earnings in 2nd quartile	0.507	40.186	0.503	40.206
Earnings in 1st quartile	0.255	42.273	0.499	40.220
Vacancy rate above the mean	0.516	42.390	0.507	42.307
Vacancy rate below the mean	0.720	42.422	0.521	42.437

Notes: Female is indicator of the gender of spouse. Age is the average age of the spouse at the time the partner is reported sick.

Table 14: Estimated effects of the WIA reform on labor participation of spouses by labor market status, quartiles of average earnings before reporting sick, and sectoral vacancy rate in the original sample and when the sample is re-weighted

	Labor participation of spouse	
	Original sample	Re-weighted sample
On permanent contract	0.001 (0.004)	-0.001 (0.004)
On temporary contract	0.025*** (0.009)	0.025*** (0.010)
Unemployed	0.010 (0.008)	0.011 (0.008)
Earnings in 4th quartile	-0.004 (0.006)	-0.017 (0.010)
Earnings in 3rd quartile	-0.002 (0.006)	-0.002 (0.007)
Earnings in 2nd quartile	0.012* (0.006)	0.019* (0.006)
Earnings in 1st quartile	0.013** (0.006)	0.013* (0.007)
Vacancy rate above the mean	0.002 (0.004)	0.002 (0.004)
Vacancy rate below the mean	0.009** (0.004)	0.011** (0.005)

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

## Appendix I: Effects of the transitional WAO reform

We analyzed the impact of changing the transitional WAO scheme to the stricter WIA scheme for the sick individuals and their spouses. Here, we analyze the impact of changing the WAO scheme to the transitional WAO scheme using the same identification strategy as described in Section 5 and used to estimate the effects of the WIA reform. Compared to the WAO, the transitional WAO made the definition of what work can still be done by the DI applicant broader. This means that a larger number of jobs were considered to match the remaining work capabilities of the sick individuals by the Employee Insurance Agency, implying tighter eligibility criteria for DI (Section 2).

Table 15 presents the estimated effects of the transitional WAO reform. Compared to the effects of the WIA reform, while the effect of the transitional WAO reform on DI awards is very similar, the effect on labor participation is much larger (cf. Table 2). Given that the WIA reform introduced multiple incentives to limit inflow and stimulate outflow, the one incentive of the transitional WAO reform to limit inflow appears to have been very effective. Compared to the WIA reform, the transitional WAO reform has a much smaller effect on UI receipt. This is in line with the large labor participation effect of the transitional WAO reform. That is, as more of the sick individuals resumed work, fewer of them made UI claims, suggesting again that the transitional WAO reform has been effective to stimulate work resumption. Figure 12 presents the dynamic effects of the reform and suggests that the common trend assumption is satisfied pre-treatment.

Unlike the WIA reform, we find no spousal response to the transitional WAO reform. There are at least two potential explanations. First, individuals who were assessed to have little or no remaining work capacities in the WAO scheme were often assessed to be well able to work in the transitional WAO scheme due to its broader definition of jobs suitable for sick individuals. This means that the WAO scheme was more lenient and there was more unused remaining work capacity among DI applicants that got activated by the transitional WAO reform that made eligibility criteria stricter. As individuals were able to increase labor participation themselves and so compensate for the lost DI benefits with earnings, there was much less need for a spousal response. Second, sick individuals who are deprived of DI in the WIA are sicker than those in the transitional WAO due to the higher minimum disability grade to become eligible for DI in the WIA (35% in the WIA versus 15% in the transitional WAO). Moreover, those who are deprived of DI in the WIA spend an additional year in SI which might have led these individuals to lose labor market attachment more than those in the transitional WAO. These mean that those who are deprived of DI in the WIA were more in need of a spousal response.

Table 15: Estimated effects of the transitional WAO reform: Sick individuals and their spouses

	Sick individual	Spouse
DI receipt	-0.028*** (0.003)	-0.003 (0.002)
Labor participation	0.018*** (0.004)	-0.001 (0.003)
UI receipt	0.003** (0.001)	-0.002* (0.001)
ln DI	-0.192*** (0.021)	-0.018 (0.013)
ln Wage	0.145*** (0.029)	-0.007 (0.025)
ln UI	0.024** (0.010)	-0.012* (0.007)
ln Total individual income	0.015 (0.025)	-0.030 (0.024)
ln Total household income	-0.009 (0.019)	
Observations	7,332,000	
Individuals	48,800	

Notes: \*\*\*, \*\*, \* denote statistical significance at 1, 5 and 10 percent, respectively. Standard errors (in parentheses) account for heteroskedasticity and clustering at the individual level. All specifications control for individual and calendar month fixed effects. The regressions use data available for the whole pre-treatment period but exclude data for the first two years of the post-treatment period.

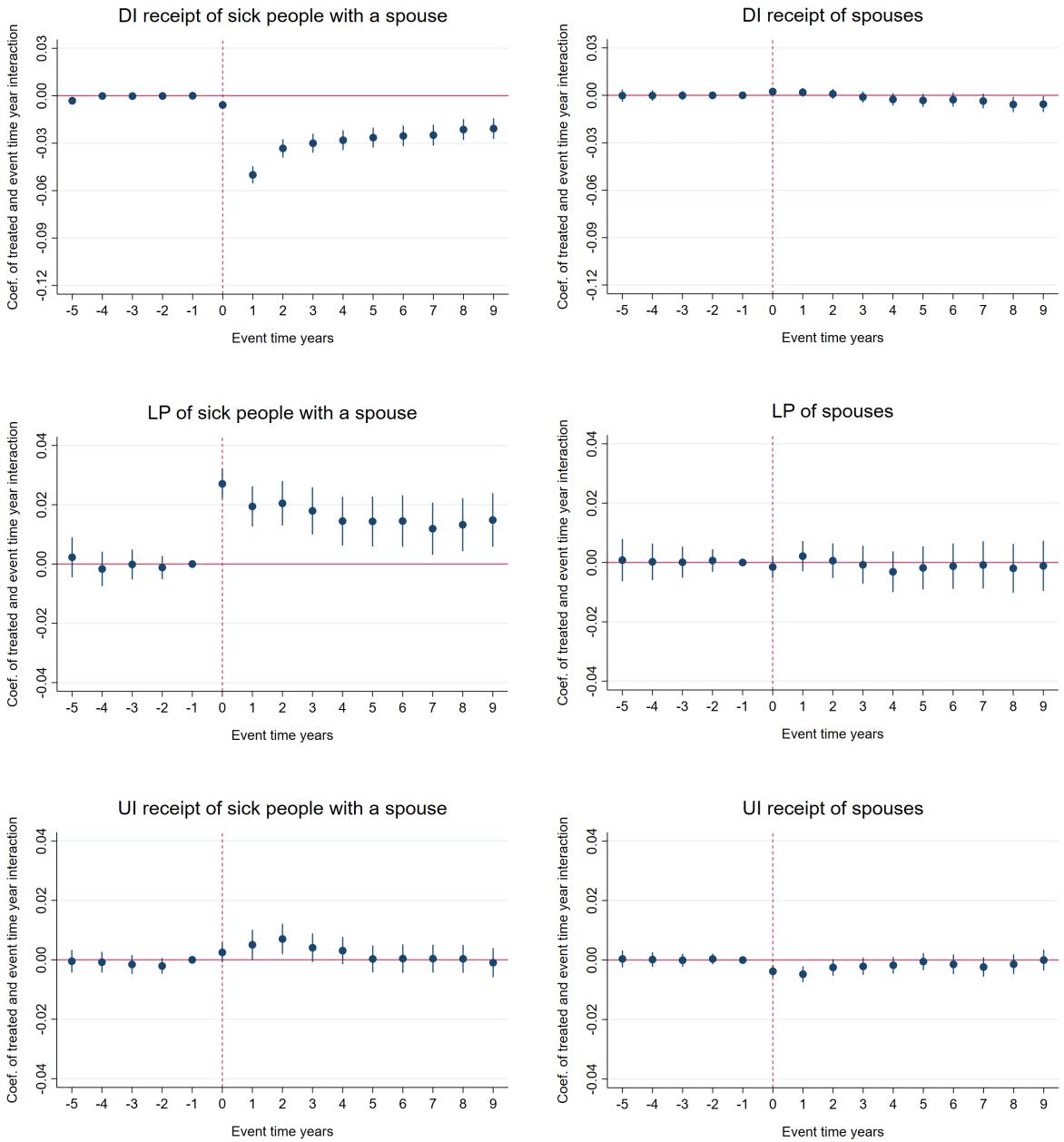


Figure 12: Estimated treatment effects of the transitional WAO reform in each of the five years before reporting sick and in each of the first ten years after reporting sick, with 95 percent confidence intervals. Observed differences between treatment and control individuals before reporting sick are controlled for using entropy balancing.